UNIVERSITY OF ALBERTA

MATHEMATICS AS MAKE-BELIEVE:
A CONSTRUCTIVE EMPIRICIST ACCOUNT

SARAH HOFFMAN ©

A thesis submitted to the Faculty of graduate Studies and
Research in partial fulfillment of the requirements for the
degree of Doctor of Philosophy

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To William, because he is the best.

Abstract

Any philosophy of science ought to have something to say about the nature of mathematics, especially an account like constructive empiricism in which mathematical concepts like model and isomorphism play a central role. This thesis is a contribution to the larger project of formulating a constructive empiricist account of mathematics. The philosophy of mathematics developed is fictionalist, with an anti-realist metaphysics.

In the thesis, van Fraassen's constructive empiricism is defended and various accounts of mathematics are considered and rejected. Constructive empiricism cannot be realist about abstract objects; it must reject even the realism advocated by otherwise ontologically restrained and epistemologically empiricist indispensability theorists. Indispensability arguments rely on the kind of inference to the best explanation the rejection of which is definitive of constructive empiricism. On the other hand, formalist and logicist anti-realist positions are also shown to be untenable. It is argued that a constructive empiricist philosophy of mathematics must be fictionalist. Borrowing and developing elements from both Philip Kitcher's constructive naturalism and Kendall Walton's theory of fiction, the account of mathematics advanced treats mathematics as a collection of *stories* told about an ideal agent and (most) mathematical objects as (mere) fictions.

The account explains what true portions of mathematics are about and why mathematics is useful, even while it is a story about an ideal agent operating in an ideal world; it connects theory and practice in mathematics with human experience of the phenomenal world. At the same time, the make-believe and game-playing aspects of the theory show how we can make sense of mathematics as fiction, as stories, without either undermining that explanation or being forced to accept abstract mathematical objects into our ontology. All of this occurs within the framework that constructive empiricism itself provides—the epistemological limitations it mandates, the semantic view of theories, and an emphasis on the pragmatic dimension of our theories, our explanations, and of our relation to the language we use.

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Introduction

Constructive empiricism owes us a philosophy of mathematics. Any philosophy of science ought to have something to say about the nature of mathematics, especially one that is mathematical in the sense that it utilizes mathematical/logical concepts like model and isomorphism. Since the central distinctions and features of constructive empiricism that turn out to be important in accounting for mathematics are derived from logical positivism, I'll briefly rehearse the history that gets us from logical positivism to constructive empiricism. This is not intended as a serious history of Twentieth Century philosophy of science; think of it as a story utilized to reveal a few of the conspicuous, and useful for my purposes, features of the landscape.

In the nineteen fifties and sixties the prevailing empiricist orthodoxy in the philosophy of science suffered a series of attacks. These eventually resulted in an overwhelming rejection of logical positivism. "Even if one is quite charitable about what counts as a development rather than a change of opinion," as van Fraassen amusingly puts it, logical positivism, "had a rather spectacular crash."[1] And there were indeed good reasons to discard logical positivism. Not least among them was positivism's restriction of meaningful sentences to empirical sentences reducible to immediately given, ostensibly defined or logical terms and names: the insistence that the "meaning of every statement of science must be statable by reduction to a statement about the given."[2] Further problems arose from the positivist syntactic and deductive characterization of scientific theories, reliance on the dubious distinction between theoretical and observation terms and claim of value-neutrality in theory choice.[3]

In the wake of the rejection of positivism, alternatives rose from various quarters, resulting in an array of different pictures of science. Popper's falsificationist methodology and rejection of any kind of inductive logic represented one alternative.[4] Kuhn's historicist account of science, introduction of the notion of a paradigm and concentration on the revolutionary character of some periods of any science's history furnished another alternative to the discarded positivism.[5] More recently, we see in Lakatos's and Laudan's focus on research programs, and in their focus on the the (ir)rationality of change in science, developments of parts of both Popper's and Kuhn's philosophies.[6]

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[1] B. van Fraassen, *The Scientific Image*, p. 2

[2] Neurath, et. al, "The Scientific Conception of the World: The Vienna Circle," p. 309. See also R. Carnap, "The Elimination of Metaphysics through Logical Analysis of Language."

[3] For exponents of the positivistic account of theories see, for example, R. Carnap's "The Methodological Character of Theoretical Concepts," *Testability and Meaning* and *Philosophical Foundations of Physics*. Also see Hempel's *Fundamentals of Concept Formation in Empirical Science*, Braithwhaite's *Scientific Explanation*, Nagel's *The Structure of Science*, and Reichenbach's *The Philosophy of Space and Time*. Examples of criticisms of the positivist account of science from

Scientific realism is another child of positivism's demise and a very successful one at that. Its core idea can be summed up as the claim that "to have good reason for holding a theory is ipso facto to have good reason for holding that the entities postulated by the theory exist."[7] Crucial to scientific realism is the rejection of positivism's use of the observation/theory distinction to avoid ontological commitments to theoretical entities. Realism insists that the language of theories is all literal, not just the observation terms, and it rejects both the idea that observation is transparent and the foundational role positivism postulates for it. Hence most of the theoretical terms of an accepted theory do successfully refer, not just the descriptive terms and names that are immediately given or ostensibly defined. Further, according to scientific realism, we are within our epistemological rights to believe that what an accepted theory tells us about the world behind the phenomena is

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around 1960 are found in Putnam's "What Theories Are Not," Sellar's "The Language ofTheory," and Feyerabend's "Explanation, Reduction and Empiricism."

[4] See Popper's *The Logic of Scientific Discovery.*

[5] See Kuhn's *The Structure of Scientific Revolutions.*

[6] Laudan, *Progress and Its Problems*, Lakatos "Falsification and the Methodology of Scientific Research Programmes, " and "History of Science and Its Rational Reconstructions."

[7] W. Sellars, *Science, Perception, and Reality*, p. 91

(approximately) true. Variants of the basic scientific realist framework can count among their advocates such philosophers as Ian Hacking, James Brown, Nancy Cartwright, Ronald Giere, Paul Churchland, Clifford Hooker, Richard Boyd, Mark Wilson and Clark Glymour.[8]

But others in philosophy of science have tried to turn back the clock, at least in a few ways. Bas van Fraassen's *The Scientific Image*, published in 1980, advocates a return to some of the doctrines logical positivism, though conceding some points to the critics of that version of empiricism. While not a full return to logical positivism, van Fraassen's constructive empiricism is explicitly framed as an alternative to scientific realism, its perceived metaphysical excess and epistemological error. In contrast to logical positivism, constructive empiricism rejects the logical analysis of scientific explanation, the construction of an inductive logic and the view of theories as interpreted formal systems. From van Fraassen's point of view, scientific explanations are to be characterized pragmatically, not simply by their syntactic and semantic features, and scientific theories are, instead of syntactic entities, to be identified with sets of models, semantic entities. This semantic theory of theories, as

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[8] See, for instance, I. Hacking's *Representing and Intervening*, N. Cartwright's *How the Laws of Physics Lie*, R. Giere's *Explaining Science*, P. Churchland's *Scientific Realism and the Plasticity of Mind*, C. Hooker's *A Realistic Theory of Science*, C. Glymour's *Theory and Evidence*, and "Explanation and Realism," R. Boyd's "Scientific Realism and Naturalistic Epistemology" and "The

Ronald Giere has pointed out, frees philosophy of science from the "linguistic shackles of its logical empiricist predecessor."[9]

Van Fraassen has written extensively on metaphysical and epistemological questions raised by philosophical reflection on science. But he has not written nearly as much on the metaphysical and epistemological issues related to mathematics. His view as an empiricist, one imagines, would be consonant with those nominalist and anti-realist philosophers who reject the reification of mathematical objects. This is in fact suggested by van Fraassen himself in an essay responding to critics of constructive empiricism. There he confesses to not having developed a philosophy of mathematics but says of the one he *would* develop:

> "I am clear that it would have to be a fictionalist account, legitimizing the use of mathematics and all its intratheoretic distinctions in the course of that use, unaffected by disbelief in the entities mathematics purports to be about."[10]

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Current Status of Scientific Realism," and M. Wilson's "What Can Theory Tell Us About Observation?"

[9] R. Giere, *Explaining Science A Cognitive Approach*, p.48. It should be noted that the semantic account of theories does not conflict with scientific realism. This view is an issue on which scientific realism and constructive empiricism can agree. Giere himself is a scientific realist who explicitly embraces a semantic account of theories that differs from van Fraassen's only in detail.

[10] B. van Fraassen, "Empiricism in Philosophy of Science," p. 283.

This thesis is a contribution to the larger project of formulating a constructive empiricist philosophy of mathematics. The philosophy of mathematics developed is fictionalist, with an anti-realist metaphysics. It makes use of elements of both Phillip Kitcher's naturalistic constructivism and Kendall Walton's theory of fiction.

Constructive empiricism owes us a philosophy of mathematics. For one thing, science is mathematical. This fact indicates that any philosophy of science ought to have something to say about the nature of mathematics. Secondly, constructive empiricism is itself mathematical in the sense that it utilizes mathematical/logical concepts like model and isomorphism. Since constructive empiricism is a philosophy of science that makes use of mathematical concepts, there had better be a way of accounting for those concepts that is compatible with constructive empiricism.

In the first chapter I defend the basic tenability of constructive empiricism. My purpose is twofold. First, developing a constructive empiricist philosophy of mathematics would have no real point if constructive empiricism were not itself plausible. So this is a necessary part of the larger project. Second, the work done defending constructive empiricism will reveal its main features, and these will serve to regulate the philosophy of mathematics as we go.

Chapter two takes a look at empiricism and the philosophy of mathematics, exploring realist thinking about mathematics—both the positive accounts and the arguments offered against the possibility of a plausible empiricist account of mathematics. These are all rejected, positive and negative together, both on general grounds and on certain grounds specific to constructive empiricism. The rejected include the realism advocated by otherwise ontologically restrained and epistemologically empiricist philosophers like Quine. In this case, rejection is based on the structural parallel between the indispensibility argument motivating the mathematical realism and inference to the best explanation arguments in the philosophy of science that are repudiated by constructive empiricism.

The chapter also considers the fortunes of anti-realist theories of mathematics, judging them once again by both general and specifically constructive empiricist criteria. Mathematics poses for empiricism arguably the most difficult of its problems. Most of the philosophy of mathematics done in the last century stems from concern with the foundations of mathematics. The main competing philosophies of mathematics of the first half of the twentieth century-logicism, formalism, and intuitionism—all address concerns raised by a feeling of crisis in the foundations of mathematics. But these concerns are different from what I take to be the main problem that mathematics raises for empiricism. I refer here to reconciling empiricist epistemology with the apparent truth of mathematical sentences.[11] Such a reconciliation seems to require violation of empiricist scruples by allowing knowledge, possibly *certain* knowledge, of objects outside any possible perceptual experience. But the alternative is evidently just as unpalatable. A rejection of mathematical objects appears to require a rejection of mathematical truth and knowledge. One of the main tasks of this dissertation is showing how that appearance is at least partly misleading.

Both logicism and formalism present mathematics in a way that promises to solve the semantic problem that mathematics raises for empiricism. Both render mathematical truth innocuous—either by reinterpreting its subject matter away, in the case of logicism, or by denying that it has a subject matter, in the case of formalism. But neither philosophy is acceptable. There are general problems with both, and constructive empiricism cannot accept them precisely because they try to simply *explain away* the semantic problem. Both have elements that ring true, however, and I aim to carry them over into the account I develop. These elements include Carnap's distinction between internal and external questions, and the formalist recognition of a game-playing dimension of mathematics, for instance. But these find expression differently and in different aspects of my account than in the theories from which they originate. The major anti-realist philosophy of mathematics from which my account borrows is Kitcher's constructive naturalism. This comes about in virtue of Kitcher's basic empiricist orientation and anti-realism about mathematical objects, but also because elements of his account answer the needs of a constructive empiricist theory of mathematics. Kitcher's account provides a starting place to respond to the semantic problem by positing a subject matter for mathematical theories that is acceptable to constructive empiricism. I adopt Kitcher's change of the domain over which mathematical variables range. Instead of abstract objects of some kind, mathematical statements quantify over the concrete operations that we perform in and on the world. While Kitcher's mathematical empiricism and naturalism provides a starting point for a constructive empiricist account of mathematics, it cannot be adopted wholesale by constructive empiricism. His espousal of a pragmatic theory of truth in reaction to the semantic problem prohibits this. An alternative development of Kitcher's basic position, one more congenial to constructive empiricism, is possible, however, and even suggested by some of his own comments. This development involves treating mathematics as *stories* and (most) mathematical objects as (mere) fictions.

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[11] P. Benacerraf discusses this problem in "Mathematical Truth,"

But a successful use of the notion of fiction to develop an anti-realist, constructive empiricist philosophy of mathematics requires that there be an acceptable anti-realist account of fiction. An analogue to the semantic problem I have described for empiricists theorizing about mathematics clearly exists for fiction. After all, we accept statements like "Sherlock Holmes smoked a pipe," with the same equanimity as statements like "Every number has a successor." It is evident that the truth of the former is likely to generate the same sort of puzzle for an empiricist as the truth of the latter. Chapter three takes up this issue and other metaphysical and logical problems that fiction raises. We cannot merely dismiss mathematical objects as fantasies; the role mathematics plays in science and the credence that we give to its truth will not let us get away that easily. The purpose of chapter three is to show that there is theory of fiction—namely Kendall Walton's make-believe theory—which not only can be used to construct an account of mathematics but that it is in fact independently the best theory of fiction currently available. Walton's theory says that a proposition is fictional if there is in some game of make-believe a prescription to imagine it. This means that a proposition can be fictional if it is true or if it is false, and allows for a semantics of fiction that does not require the existence of fictional objects of any kind. As in chapter one's treatment of constructive empiricism, the aim here is twofold. First I give a general defense of the theory, especially against realist alternatives. Second I outline the main features of the theory, not only as Walton himself articulates it but also through the eyes of speech act theory, which is how I argue Walton should be read. Using speech act theory to interpret the make-believe theory of fiction not only makes it easier to explain how we can say things like "every number has a successor" without being committed to their truth, like constructive empiricism does with scientific theories, it also emphasizes the pragmatic dimension of our acceptance and use of mathematics.

The completion of this prepatory work sets the stage for the final chapter in which a constructive empiricist philosophy of mathematics is outlined. Together with elements of Kitcher's theory of mathematics, the make-believe account of fiction generates a constructive empiricist view of mathematics. The naturalism adopted from Kitcher explains what the true portions of mathematics are about and why mathematics is useful, even while it is a story about an ideal agent operating in an ideal world. It connects theory and practice in mathematics with human experience of the phenomenal world. The make-believe and game-playing aspects of the theory show how we can make sense of mathematics as fiction, as stories, without either undermining that explanation or accepting abstract mathematical objects into our ontology. All of this occurs within the framework that constructive empiricism itself provides—the epistemological limitations it mandates, the semantic view of theories, and an emphasis on the pragmatic dimension of our theories, our explanations, and of our relation to the language we use.

The conclusion that mathematics is make-believe may strike some as preposterous. In fact, my project may lead them to a negative conclusion: Hoffman's account of mathematics provides one more good reason to reject constructive empiricism. Anyone is, of course, free to draw this conclusion. But my view is more positive. That the account links the human representational activities of science and art and mathematics seems to me an advantage. That it allows us to recognize more dimensions to our relationship with the language we use to make our way through the world than the two of belief and disbelief strikes me as a greater one. As does the recognition of the fundamental role of imagination and make-believe in mathematics and science.

Chapter One

Constructive Empiricism and the Case Against Scientific Realism

The picture of science presented by van Fraassen addresses several standard questions about science. What are scientific theories? How does science explain? What is the aim of science? But possibly the most contentious aspect of the picture he offers is the limit it sets on scientific knowledge. This is dictated for van Fraassen by a properly empiricist attitude towards science:

> To be an empiricist is to withhold belief in anything that goes beyond the actual, observable phenomena. To develop an empiricist account of science is to depict it as involving a search for truth only about the empirical world, about what is actual and observable.[12]

Withholding belief in this way clearly violates the spirit of scientific realism. Science in that philosophy is depicted as a response to the "the demand for an explanation of the regularities in the observed course of nature, by means of truths concerning a reality beyond what is actual and observable."[13] While this exact way of putting the matter may not be thought best by some scientific realists, it is clear from realists' own portrayals that they oppose the sort of belief withholding van Fraassen has in mind. Giere for one characterizes scientific realism as "the view that when a scientific theory is accepted, most elements of the theory are taken as representing... aspects of the world."[14] Putnam describes scientific realism as the view that the sentences of scientific theories are true or false, that what makes them true or false is something external, and that the theories of a mature science are normally (approximately) true.[15] And Boyd, in characterizing the picture of science that scientific realism presents says that

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[12] B. van Fraassen, *The Scientific Image*, p. 202

[13] B. van Fraassen, *The Scientific Image*, p. 203

> Scientific knowledge extends to both the observable and the unobservable features of the world... the operation of the scientific method results in the adoption of theories which provide increasingly accurate accounts of the causal structure of the world.[16]

So for scientific realists the aim of science regarding theories is truth, full stop, not merely truth about observables. "In the dimension of describing and explaining the world, science is the measure of all things, of what is that it is, and of what is not that it is not."[17]

Constructive empiricism, on the other hand, does not identify wholly true theories as the ultimate aim of science. Science can be fully satisfied with less. When we accept a scientific theory we are required to go no further in belief than the limits of what the theory says about what is observable, the limits of its empirical content. And fully acceptable theories need only be true to the limits of their empirical content - any truth beyond that is supererogatory. "Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate... a theory is empirically adequate exactly if what it says about the observable things and events in the world, is true."[18] Unlike many instrumentalists, however, the constructive empiricist does not insist on a non-literal understanding of the fragments of the language in theories that talk about unobservables. In this limited respect at least, scientific realism and constructive empiricism agree. The language of theories, including that about unobservables, is to be literally construed. Claims about unobservables in any theory, understood to say just what they seem to say, may in fact be true.[19] The difference lies in the status of all that truth as a goal of science.

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[14] R. Giere, *Explaining Science A Cognitive Approach*, p. 7

[15] H. Putnam, *Mathematics, Matter and Method*, p. 69-74. Putnam attributes the first part of the idea to Michael Dummett, the second to Richard Boyd.

[16] R. Boyd, "Scientific Realism and Naturalistic Epistemology," p. 613

[17] W. Sellars, *Science, Perception, and Reality*, p. 173.

Where does this leave us then? The anti-realism of constructive empiricism about unobservables is, we might say, of the agnostic sort. [20] That is, constructive empiricism does not say that there are no electrons (or whichever unobservable you like), only that science cannot give us good enough reason to believe that there are.[21] But, as is apparent from constructive empiricism's view of the ultimate aim of science, this should not be thought of as a failing on the part of science. Indeed, constructive empiricism says, we will understand science better if we do not make the mistake of thinking that it is part of its job to give us reasons to believe that there are electrons. Science only aims for empirically adequate theories.

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[18] B. van Fraassen, *Scientific Image*, p. 12.

[19] "The idea of a literally true account has two aspects: the language is to be literally construed; and so construed, that the account is true. This divides anti-realists into two sorts. The first sort holds that science is or aims to be true, properly (but not literally construed). The second holds that the language of science should be literally construed, but its theories need not be true to be good. The anti-realism I shall advocate belongs to the second sort." B. van Fraassen, *Scientific Image*, p. 10.

[20] This antirealism is not exactly the same for all unobservables. Van Fraassen maintains agnosticism about the unobservable entities involved in empirically adequate theories while professing what we might call atheism about physical laws, for instance. But this atheism is based on more than the arguments considered here.

[21] Unless and until electrons become part of the class of observables. Constructive empiricism allows for this possibility. Our epistemic community may expand to include beings for whom electrons are observables, for instance. In such circumstances, however, science would not be giving *us* good enough reason to believe that there are electrons, but a new expanded epistemic community.

A theory is acceptable to the extent that it is empirically adequate, and empirically adequate to the extent that what it says about the phenomena is true. Alternatively, a theory is empirically adequate if all of the phenomena fit into at least one of its models. Just this much truth can satisfy the purposes of science: the truth about the phenomena. Since the phenomena are exhausted by what is observable, constructive empiricism holds that the truth science is concerned to find is not about unobservable parts of the world. The aim of science can be fully achieved even by a theory that falls short of complete truth, by one for which not every element in its models corresponds to something in the world.

Here sits the conflict with scientific realism. We have seen that for scientific realism accepted theories are taken to be more or less true. The truth of scientific theories is not in any way limited to just the observables involved in the theory. So the scientific realist must insist that the aim of science is more than empirical adequacy; he must insist that the aim is theories all of whose elements - not just those referring to observables - correspond to something in the world. If a theory only achieved empirical adequacy and was otherwise false it would not ultimately be good enough for a realist, for it would not reveal the way the world behind the phenomena *really* was. It would not be a theory that we could correctly believe to tell us the whole truth about its subject matter; hence our acceptance of it would have to be to that extent qualified. But constructive empiricists can without qualification accept such a theory. For them, science is neutral about truth beyond the observable.

Although I have been framing it as a dispute over the aims of science, an epistemological disagreement is what really lies at the heart of conflict between scientific realism and constructive empiricism.[22] Considering the argument generally advanced for scientific realism illustrates this. A variety of arguments have been developed for scientific realism, but most share a reliance on some form of inference to the best explanation.[23] Many start with unexceptionable observations about science. Theories in science, we are told, are accepted or rejected partly on the basis of how well they explain the evidence or data. Scientists look for theories that not only predict the regularities that they study but also *explain* those regularities. And moreover, how well a theory explains is a partial measure of how acceptable it is, how likely it is to be true. We think that the explanatory power of General Relativity, for instance, is reason to think that it is true. It explains, this is evidence of its truth. Perhaps better: It provides the best explanation of a host of things in the world, and *this* is reason to think that it is true.

To bring this a little closer to the ground, consider a kind of inference we all make as a matter of course in our everyday lives. We are presented with evidence of the mousely sort; there is missing cheese, a damaged phone cord, scrabbling in the walls. From this data, without ever actually seeing a mouse, we infer that there is a mouse about. The inference is from a certain set of evidence to the truth of a theory that both goes beyond and explains that evidence. Our belief that there is a mouse is based on the fact that it is the best

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[22] Sober points out the epistemological nature of the disagreement in "Constructive Empiricism and the Problem of Aboutness." His judgement is clearly born out in the voluminous debate in the literature regarding the nature and legitimacy of ampliative inference, inference to the best explanation. A sample of those addressing the epistemological issues include D. Nelson "Confirmation, Explanation and Logical Strength," P. Forrest "Why Most of Us Should Be Scientific Realists," S. Leeds ""Constructive Empiricism," A. Kukla "Does Every Theory Have Empirically Equivalent Rivals?" B. Ellis "What science aims to do," D. Hausman "Constructive Empiricism Contested," and Laudan and Leplin "Empirical Equivalence and Underdetermination"

[23] Including, but not restricted to, J. J. C. Smart *Between Science and Philosophy*, Wesley Salmon "Why ask why?" W. Sellars "Is scientific realism tenable?" R. Giere "Explaining Science," C. Glymour "Explanations, Tests, Unity and Necessity," and R. Boyd ""Scientific Realism and Naturalistic Epistemology," and "Lex Orandi est Lex Credendi."

explanation of the missing cheese, the chewed-through phone cord and the scrabbling in the walls. The mouse theory is the best explanation of the data - we infer to the truth of the best explanation. We do not infer, take note, the truth of "all observable phenomena are as if there is a mouse" but the truth of the stronger "there is a mouse."

Perfectly ordinary, perfectly justified, and the reasoning practices that inferences of this sort constitute lead us to scientific realism. How do they do this? Well, let's look at the argument put forward for scientific realism. "Both scientific realists and (almost all) empiricists agree that [the methodological practices of science] are instrumentally reliable, but they differ sharply in their capacity to explain this reliability."[24] Boyd contends that "scientific realism provides the only scientifically reasonable explanation for the reliability of certain important features of scientific methodology."[25] So he claims that a good reason to believe scientific realism is that it provides the best explanation of the reliability of scientific methodology. This argument asks us to infer from the instrumental success of science to the truth of scientific realism. If scientific realism were not true, then how else could we explain the successes of science? That is, the argument for realism holds that the instrumental success of science entails that accepted theories are (approximately) true, belief in the entities mentioned in those theories is sanctioned, and that accepting a theory means accepting it as (approximately) true.

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[24] R. Boyd, "Lex Orandi est Lex Credendi," p. 13.

[25] R. Boyd, "Lex Orandi est Lex Credendi," p.4. For more on this and related material see also his "Realism, Underdetermination and a Causal theory of Evidence," "Scientific reasoning and Naturalistic Epistemology," and "On the current status of scientific realism." I. Hacking, *Representing and Intervening* and N. Cartwright, *How the Laws of Physics Lie* also present arguments from scientific methodology for scientific realism, though of a more restricted kind.

Take note, however, that this argument has the same inferential structure as the inference that is the *content* of scientific realism. The instrumental success of a scientific theory is said to be evidence for the truth of that theory, because the truth of the theory best explains the evidence or data. And, in turn, the truth of a philosophical theory (scientific realism) is shown by the explanation it provides of another datum: the success of science. It is, however, exactly the inference from the instrumental success of a theory to the truth of claims it makes about unobservables that is disputed by constructive empiricism. It is true that, if scientific realism is correct, it explains the success of the techniques and methods that science uses, that it is perhaps even the best explanation of said success.[26] But inferring that scientific realism is true from this begs the question against constructive empiricism. To find the argument compelling you must already accept inference to the best explanation.

More generally, as van Fraassen has pointed out, the problem for scientific realism is that explanatoriness is not connected to truth in a way that would make inference to the best explanation generally legitimate.

> In so far as they go beyond consistency, empirical adequacy, and empirical strength...[virtues claimed for a theoery] provide reasons to prefer the theory independently of questions of truth... To praise a theory for its great explanatory power, is therefore to attribute to it *in part* the merits needed to serve the aims of science. It is not tantamount to attributing to it special features which make it more likely to be true, or empirically adequate.[27]

And this is true despite its apparent conflict with some of our everyday reasoning practices - those illustrated by the mouse theory case. Mouse cases, and their analogues, are raised by realists aiming to establish that we really do accept and practice inference to the best explanation. If they are right and we really do, there has to be something wrong with van Fraassen's insistence that explanatory power is merely a pragmatic virtue and inference to the best explanation must be rejected. However, it is clear that mouse cases do not support the generally legitimacy of inference to the best explanation, when we notice what conclusion *really* ought to be drawn from cases of this sort.

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[26] But what it won't explain, as Larry Laudan has pointed out, is the long history of cases in science where the best explanation has since been shown false. See Laudan "A Confutation of Convergent Realism."

The crucial question to consider is whether the mouse case is an instance of inference to belief in the truth of a theory, or, rather, that it is a case of inference from evidence to only theory acceptance. Is it an inference to truth or to empirical adequacy? Consideration of these alternatives quickly shows that for the mouse case, they really are not actually alternatives at all - they amount to the same thing. Such cases "cannot provide telling evidence between these rival hypotheses."[28] This is because the mouse theory is a case of a theory strictly about observables, and for such theories acceptance and full belief are exactly the same thing. The theory "There is a mouse" is empirically adequate if and only if it is true. Thus, this is not a case in which an inference is being made to truth beyond empirical adequacy. So, even if we do infer to the truth of a theory in such cases, this cannot establish that a parallel inference is allowable in cases of theories where whole truth does go beyond empirical adequacy, those involving unobservables. Cases where empirical adequacy and full truth coincide support an alternative to inference to the (truth) of the best explanation; they equally

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[27] B. van Fraassen, *Scientific Image*, p. 88-9

[28] B. van Fraassen, *Scientific Image*, p. 21.

support the principle to be "willing to believe that the theory which best explains the evidence, is empirically adequate."[29]

Much of the justification for making such inferences at all must be that, when we have come to such beliefs in the past, subsequent evidence has shown the theory true. In other words, in the past there has been an actual mouse-sighting on the heels of evidence of a mously sort. But such confirmation is, by definition, not available in the case of theories involving unobservables—given the epistemological weight that van Fraassen places on the observable/unobservable distinction, anyway; but the status of the distinction is a problem I must put off for now. The best we can hope for with theories involving unobservables is good confirmation of the truth of what they say about the observables. It seems that approaching scientific realism through inference to the best explanation is question begging against the anti-realist.

Ian Hacking proposes a different route.[30] Hacking intends to go in through experiment. But his use of scientific practice to argue for realism is intended not to parallel Boyd's use of scientific methodology. Hacking's experimental realism purports to give reasons for belief in unobservable entities that do not rely on any kind of inference to the best explanation. Hacking's is an attenuated realism, one with a more strictly circumscribed set of entities in which it sanctions belief. His arguments for a version of scientific realism turn the debate away from talk about theory and towards experimentation. He shares this general approach, and the resultant entity realist conclusions, with Nancy Cartwright.[31] Recognizing the way in which realist and anti-realist arguments often talk past each other, Hacking constructs an "experimental argument for scientific realism."[32] In fact, what he has are two different arguments for realism about entities.[33] One pertains to tiny yet observable entities and the other entities that in principle cannot be observed. In both cases the argument concludes that there are instances of these entities to which we have theory independent access. Hacking stresses theory independence because he agrees that arguments for scientific realism based on inference to the best explanation are question begging against the anti-realist. Hence the turn to experiment. Unfortunately, for scientific realism, anyway, neither of Hacking's arguments can deliver on their promise.

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[29] B. van Fraassen, *Scientific Image*, p. 20.

[30] I. Hacking, *Representing and Intervening*, and "Do we see through microscopes"

[31] N. Cartwright, *How the Laws of Physics Lie*.

The first argument turns on the fact that we are able to produce images of microscopic entities that agree using a variety of different instruments which operate according to different physical processes: pictures of, say, some of the internal structure of a cell. Hacking argues that this is evidence that our instruments give us theory-independent access to (certain) unobservable entities, and, further, that we have independent access to these entities gives us good reason, he thinks, to conclude that they are real and not artifactual. "It would be a preposterous coincidence if, time and again, two completely different physical processes produced identical visual configurations which were, however, artifacts of the physical processes rather than real structures."[34] But a clear flaw is apparent in this argument - it invokes explanatoriness as an indicator of truth. The claim of preposterous coincidence implies that there are two possible explanations for the data. Either the visual configurations are an artifact or they are real structures. The reason put forward to substantiate the claim that we ought to believe the structures are real is that the artifactual explanation is inferior to the explanation that they are real - it would, after all, make the visual configurations a preposterous coincidence. This is, however, just another invocation of inference to the best explanation. So much for the first argument.

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[32] I. Hacking, *Representing and Intervening*, p. 145.

[33] A fact pointed out in R. Reiner and R. Pierson,, "Hacking's Experimental Realism: An Untenable Middle Ground."

[34] I. Hacking, *Representing and Intervening*, p. 201

Let's see if the second fares any better. Manipulation of unobservable entities is the keystone of Hacking's second argument. In this argument 'real' is contrasted with 'merely a tool of thought' (rather than artifactual, as in the first argument). Certain entities in science, usually those we take ourselves to know the most about, are used as instruments to manipulate and learn about entities we know less about. Scientists have skills by means of which they use certain unobservable entities to manipulate other unfamiliar unobservable entities. These skills, the argument goes, constitute access to unobservable entities. And they are independent of the truth of any particular theory. Theories may come and go but the laboratory techniques with which scientists manipulate entities can be detached from any theoretical knowledge. "One needs theory to make a microscope. You do not need theory to use one."[35]

Further, while merely experimenting on an entity does not commit you to a belief that it is real, "manipulating an entity, in order to experiment on something else, need do that."[36] Using unobservable entities as instruments involves us in conferring on them the highest possible degree of belief in their existence. The practice cannot be made sense of in the context of withholding belief. If the instruments we use are not real, then surely it is irrational for us to expect that we can actually use them to do anything, let alone use them reliably.

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[35] I. Hacking, *Representing and Intervening*, p. 191

Again, however, Hacking's argument cannot convince the anti-realist. His presumption is that some experimental practices give us theory independent access to unobservable entities. This cannot be established to the satisfaction of an anti-realist. Laboratory skills give us access "only to certain observable interactions in the apparatus." And, more importantly, only by inference to the best explanation "can we come to believe that these observable signs indicate the presence of causal interactions, that these interactions are not artifacts, and that the entities lie behind them."[37] An implicit appeal to explanation - explanation of the observable interactions in the apparatus - is what moves the argument. But *this* is not a non-question-begging reason to prefer the realist conclusion to the conclusion that what grounds the use of the kind of laboratory techniques Hacking points to is that these practices and the theories they generate are merely empirically adequate. Hacking's second argument has not provided a reason to prefer scientific realism to constructive empiricism.

Nancy Cartwright's argument is another variation on the inference to scientific realism. She contends that while van Fraassen's arguments against inference to the best explanation are persuasive, there is a class of these inferences that escape from his objection. First, the inferences that do not escape: Inferring that a theory saves the phenomena from its success at saving the phenomena is legitimate, but to further conclude that the theory is true would be unwarranted. It would constitute a misunderstanding of explanation. Theoretical explanations do not require the truth of theoretical principles, only that the explanandum be derivable from those principles. "Explanations... organize, briefly and

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[36] I. Hacking, *Representing and Intervening*, p. 263

[37] R. Reiner and R. Pierson, "Hacking's Experimental Realism: An Untenable Middle Ground," p.67

efficiently, the unwieldy, and perhaps unlearnable, mass of highly detailed knowledge that we have of the phenomena. But organizing power has nothing to do with truth."[38]

Now to the exceptions to this reasoning: the class of explanations that Cartwright thinks do not follow this pattern. These are causal explanations. Causal explanations do not invoke laws that help to organize, but invoke causes, often specific unobservable entities.[39] When we say that C causes E in explanation of E we are warranted in inferring that C exists. For if C did not exist how could it cause E? Cartwright explains that causal explanations fall outside the scope of the anti-realist object because, while truth is not an internal characteristic of theoretical explanations, it *is* an internal characteristic of causal explanations. In order to explain at all causal claims must be true. Inference to the best explanation in causal explanations is still inference to the best explanation, but, Cartwright maintains, a legitimate form of inference to the best explanation.

However, this isn't quite right. Causal claims are dependent on scientific theories, and when causal claims involve unobservables then so too must the theories that generate them. Without belief that a theory is true, which the constructive empiricist rejects, we do not have enough reason to believe the causal claims it begets to be true. At best, they are shorthand for the kind of explanation the accepted theory provides - predictive and organizational. Cartwright herself recognizes this problem, saying that the "fact that causal hypotheses are part of a generally satisfactory explanatory theory is not enough, since success at organizing, predicting, and classifying is never an argument for truth."[40] What is enough to give causal claims the necessary robustness and independence from theory, she thinks, is the practice of direct experimental testing.

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[38] N. Cartwright, *How the Laws of Physics Lie*, p. 87

[39] Cartwright uses the phrase 'theoretical entity," but I use 'unobservable' for two reasons. One, it makes clear the continuity of Cartwright's discussion with van Fraassen's work. Second, it seems to me that absent a vocabulary uncontaminated by theory, both observable and unobservable entities are 'theoretical.' The disagreement between van Fraassen and realists, in particular Cartwright, is not over *theoretical* entities but *unobservable* ones.

So we are back to experimentation again. Scientists manipulate causes, looking to see if their effects change in the predicted manner. Often, scientists have developed their ability to manipulate unobservable entities in incredibly subtle and detailed ways, allowing intervention in other processes. And this practice only makes sense against the background of the truth of scientists' beliefs about the unobservable causes that they manipulate. If they were wrong, how could they have such skill? This sounds suspiciously familiar. And for good reason: we are back to one of the arguments that Hacking makes.[41] Our ability to manipulate certain unobservable entities is the purported ground of our belief in their existence. However, I have already argued that Hacking's argument relies on inference to the best explanation. I conclude that Cartwright's does as well. Neither establishes that their entity realism is better warranted than other kinds of scientific realism. They stand or fall together; all rely on inference to the best explanation.

The constructive empiricist and realist appear talking past each other here, one denying and the other embracing inference to the best explanation. The kinds of arguments that I have been discussing cannot by themselves decide the merits of the two positions. We need to look more closely at the grounds for accepting or rejecting inference to the best explanation.

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[40] N. Cartwright, *How the Laws of Physics Lie*, p. 98

[41] Cartwright clearly recognizes this, saying on p. 98 "I agree with Hacking that when we can manipulate our theoretical entities in fine and detailed ways to intervene in other processes, then we have the best evidence possible for our claims about what they can and cannot do."

Challenging Constructive Empiricism

The spirit motivating constructive empiricism's rejection of inference to the best explanation is conservatism—believe as little as you are forced to, change your beliefs as little as possible, in order to get what you need. In the case at hand, what we need as philosophers is a credible account of the assortment of interventions, activities and constructions that we call science. Perhaps, then, the dispute between scientific realism and constructive empiricism is really about just how conservative we can get away with being, while still remaining credible. Some of the direct challenges issued to constructive empiricism suggest that this is the right way to view the matter. A familiar realist complaint about constructive empiricism is that it provides an unsatisfactory picture of science. The grounds cited for this claim range from the complaint that constructive empiricism can't make sense of the doxastic attitudes of real scientists, never mind their actual practices[42], to the objection that it rests on distinctions that cannot coherently be maintained.[43]

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[42] Boyd argues that "the consistent empiricist cannot even justifiably conclude that the methods of science have been instrumentally reliable in the past, much less that they will be reliable in the future." ("Lex Orandi est Lex Credendi," p.32) Chihara and Chihara maintain that "the Rejection of Unobservables Thesis is not plausible when applied throughout biology." ("A Biological Objection to Constructive Empiricism," p. 654) In addition, Hacking's and Cartwright's arguments for entity realism from experimentation and causal reasoning in science equally imply an objection that constructive empiricism is deficient in its picture of science. (see I. Hacking "Do We See Through Microscopes" and *Representing and Intervening*, and N. Carwright *How the Laws of Physics Lie*.)

[43] On the coherence of constructive empiricism see M. Freidman, "Review of Bas Van Fraassen's *The Scientific Image*," S. Leeds, "Constructive Empiricism," M. Wilson, "What Can Theory Tell Us About Observation?" A. Musgrave, "Realism Vs. Constructive Empiricism," S. Mitchell, "Constructive Empiricism and Anti-Realism," P. Horwich, "On the Nature and Norms of Theoretical Commitment," P. Churchland "The Anti-Realist Epistemology of Van Fraassen's '*The Scientific Image*,'" V. Harcastle, "The Image of Observables," and J. Foss "On Accepting Van Fraassen's Image of Science."

Van Fraassen's argument is that there are no grounds for believing that explanatory power is connected to truth. He takes "truth to be an external characteristic to explanation." And challenges the scientific realist to "tell what is special about the explanatory relation."[44] There are many equally explanatory theories that go beyond the phenomena in differing ways. Only one can be true, so the probability of the one we have, among all the others that are equally explanatory that we don't have, being the true one must be very small. So we ought not to believe that the explanatory theories we have are the true ones. But is this a legitimate inference? Don't scientists believe their own theories and aren't they justified in doing so? Van Fraassen's account of science appears to make the beliefs and practices of scientists seem rather strange. He seems to be saying that they shouldn't do what they do. Van Fraassen does insist that "[f]or belief... all but the desire for truth must be 'ulterior motives'."[45] So it might seem that his account cannot allow for scientists' behavior.

In science theories are often pursued, even when belief in them seems radically under-justified. Constructive empiricism should be able to say something about why this is the case, and why scientists are justified in doing this. Science looks for not just truth but significant truth. Even so, this presents no problem for van Fraassen. That truth is the *only* goal of scientific inquiry does not follow from the claim that desire for truth is the only legitimate motive for belief. Unpacking 'significant' reveals an assortment of values--simplicity, power, elegance and perhaps others. Science pursues these, or maybe some slightly different set, in addition to truth. Still, to think that constructive empiricism is not in a position to account for this ignores the work that acceptance can do. One does not have to believe a theory is true to believe that it is simple, elegant or powerful. Of course, it will fail on at least some of these counts if it is not empirically adequate. But this does not trouble constructive empiricism; empirical adequacy is exactly the aim that it imputes to science. Others are not denied, just others that involve truth beyond empirical adequacy. If anything, it is scientific realism that experiences difficulty on this point. How, if truth beyond empirical adequacy is really the aim of scientists, can we account for their dedication to the pursuit of values that will interfere with the attainment of that goal?

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[44] N. Cartwright, *How the Laws of Physics Lie*, p. 91

[45] B. van Fraassen, *Laws and Symmetry*, p. 192

Acceptance can in fact do all the work here for which the scientific realist seems to think belief is needed. It is the appropriate response to a theory that speaks to our desires for informativeness, simplicity, elegance, and potential fruitfulness in addition to our desire for empirical adequacy. These virtues provide truth-independent reasons to choose a theory. Van Fraassen's point is that this choosing should not be construed as a choice to believe, but only a choice to accept as empirically adequate. However, this response relies on the tenability of the distinction between believing and merely accepting a theory, a distinction that itself depends on another, that between observable and unobservable entities. It further depends on the claim that virtues like simplicity and potential fruitfulness detract from the likelihood a theory is true. And challenges to constructive empiricism on both these grounds have been made.

Examples of arguments characteristic of this kind are found in Stephen Leeds's "Constructive Empiricism." He raises two general questions: Can it be convincingly argued that we ought not to believe what accepted scientific theories tell us about unobservables? Can an account that holds attributions of observability as matters to be decided by science be coupled with skepticism about unobservables? "No" is Leeds's answer to both. The first engages constructive empiricism on its home turf, the issue of inference to the best explanation. On this count, I will argue, Leeds does not undermine constructive empiricism's stance that we need not believe what theories tell us about unobservables. He argues that van Fraassen needs an argument that scientific theories are in general unlikely to be true, and that this cannot be substantiated. But Leeds's does not make his case. However, the second question presents a more serious challenge. The coherence of constructive empiricism is at stake with this one. It also represents a criticism that is more important for my project, since the way it fails is by ignoring the difference between talking about a theory and talking about the world. This difference is going to turn out to be important in developing a constructive empiricist philosophy of mathematics. But first, to the question of belief.

Leeds's rejects the assertion that virtuous theories are unlikely to be true. This strategy for undermining is not so clearly question begging as those involving inference to the best explanation that I have already discussed. At least it looks possible that the strategy engages the debate with constructive empiricism and does not just talk past it. The argument that theories are in fact not unlikely to be true goes after constructive empiricism at a place logically prior to the issue of inference to the best explanation, a place where realism and empiricism may share enough that there is common ground to argue on. We shall have to see if this argument succeeds where others have failed.

Leeds's identifies the claim that "such theoretical virtues as simplicity, informativeness, and explanatory power, which play so large a role in our coming to accept theories that posit unobservables, are virtues which detract from the likelihood of a theory" as the central argument for constructive empiricism.[46] While characterizing this assertion as *the* central argument for constructive empiricism seems wrong, the assertion is an important part constructive empiricism's rejection of scientific realist arguments. Van Fraassen sees for constructive empiricism a more positive role than Leeds appears to allow; the assertion argues against scientific realism. More is claimed in favor of constructive empiricism than mere consistency with the claim that theoretical virtues detract from the likelihood of a theory. In *The Scientific Image* we find van Fraassen saying that "...there is also a positive argument for constructive empiricism - it makes better sense of science, and of scientific activity, than realism does and does so without inflationary metaphysics."[47]

A fuller assessment of constructive empiricism than Leeds provides would evaluate the extent to which van Fraassen's claim here is born out by the evidence. The second part of the claim must be true since it is precisely the avoidance of inflationary metaphysics at which constructive empiricism aims. But more than this is necessary to show that constructive empiricism makes better sense of science and scientific activity than does realism. Leeds hints that the case can't be made, but doesn't take up the argument.[48] Nevertheless, it is worth taking up Leeds's remark that there is only a slim chance of the kind of project van Fraassen undertakes in *Laws and Symmetry* can be equally successful if applied to unobservable entities.

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[46] S. Leeds, "Constructive Empiricism," p. 199-200. Many others have also identified the argument as central to constructive empiricism. Challenges on these grounds have been mounted by A. Musgrave "Realism Vs. Constructive Empiricism," M. Wilson "What Can Theory Teach Us About Observation," R. Giere, "Constructive Empiricism," P. Forrest "Why Most of Us Should Be Scientific Realists," D. Nelson "Confirmation, Explanation and Logical Strength," C. Glymour "Explanations, Tests, Unity and Necessity," R. Boyd "Realism, Underdetermination, and A Causal Theory of Evidence," and P. Churchland ""The Ontological Status of Observables: In Praise of the Superempirical Virtues." Discussions more sympathetic to van Fraassen on this point include N. Carwright *How the Laws of Physics Lie*, B. Ellis "What Science Aims To Do," and A. Kukla "Does Every Theory Have Empirically Equivalent Rivals," and "Non-Empirical Theoretical Virtues and the Argument From Underdetermination."

[47] p. 73

In *Laws and Symmetry* there is a sustained argument against realism about laws of nature. Van Fraassen persuasively argues that the concept of a law of nature is unclear and that any account of laws of nature must satisfy two jointly unsatisfiable conditions: first, that it is a law that A should imply that A is the case, and second, the sort of fact about the world that gives 'law' its sense, and distinguishes between laws and mere regularities must be specified.[49] This expression of the two conditions for a satisfactory account of laws is rudimentary; it in no way establishes the essential conflict between them. The argument for this is contained in *Laws and Symmetry*. Van Fraassen there also makes a good case that laws of nature, any way the concept has been explicated, really do not play a role in science. Altogether this constitutes a very strong case against realism about laws. Now, what Leeds appears to claim is that van Fraassen has not and probably cannot make such a case against realism about unobservables. Fair enough, but van Fraassen has neither need nor desire to make a case like this. His aim in *Laws and*

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[48] At the close of his critical article on constructive empiricism, Leeds says "The first half of *Laws and Symmetry*... shows that there are difficulties in defining what a law is, in identifying which are the laws, and in finding them a role to play. It would be interesting to see how one might go about constructing a similar argument against belief in atoms..." ("Constructive Empiricism," p. 217)

[49] "...that it is a law that *A*, should imply that *A*, on any acceptable account of laws. We noted this under the heading of necessity. One simple solution to this is to equate *It is a law that A* with *It is necessary that A*, and then appeal to the logical dictum that necessity implies actuality. But is 'necessary' univocal? and what is the ground of the intended necessity, what is it that makes the proposition a necessary one? To answer these queries, one must identify the relevant sort of fact about the world that gives 'law' its sense; that is the problem of identification." (B. van Fraassen, *Laws and Symmetry*, p 38-9)

Symmetry was to show that there are no laws of nature. His aim regarding unobservables has never been to show that there are none.

Van Fraassen does not claim that unobservables play no role in science, he does not maintain that the concept of unobservables in unclear, nor does he assert that any account of unobservables has to satisfy a set of jointly unsatisfiable conditions. Why doesn't he do these? Well, because he is not an atheist about unobservables. He is an atheist about laws of nature, he is sure that there are none of these, but about unobservables he is merely agnostic.[50] Thus the kind of argument he needs to make needn't have as strong a conclusion as the one against laws. What van Fraassen instead needs to do is make the case that the role that unobservables do play in science does not require of them that they exist.

Nevertheless, we do find van Fraassen explicitly proposing that some theoretical virtues detract, or at least do not enhance, the likelihood of a theory's truth. He tells us, about theories, that it is "an elementary logical point that a more informative theory cannot be more likely to be true..." and "...reasons for acceptance include many which, *ceteris paribus*, detract from the likelihood of truth. In constructing and evaluating theories, we follow our desires for information as well as our desire for truth."[51] Similar statements appear elsewhere as arguments for constructive empiricism. For instance:

> There are a number of reasons why I advocate an alternative to scientific realism... One concerns the

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[50] This difference of treatment might be viewed as a bit of concession to the entity realism of Hacking and Cartwright. Though it does not reach the level of realism about unobservable entities, it does recognize something like the difference that Cartwright highlights in saying that van Fraassen's arguments that inferences to pure theory justified in terms of explanation are persuasive, but that "[a]rguments against inference to the best explanation do not work against the explanations that theoretical *entities* provide." (*How the Laws of Physics Lie*, p. 89. My italics.)

[51] B. van Fraasssen, *Laws and Symmetry*, p. 192

> difference between acceptance and belief; reasons for acceptance include many which, *ceteris paribus*, detract from the likelihood of truth... It is an elementary logical point that a more informative theory cannot be more likely to be true: therefore the desire for informative theories creates a tension with the desire to have true beliefs.[32]

And elsewhere:

> ...[O]ther virtues claimed for a theory are pragmatic virtues. In so far as they go beyond consistency, empirical adequacy, and empirical strength, they do not concern the relation between the theory and the world, but rather the use and usefulness of the theory; they provide reasons to prefer the theory independently of questions of truth.[33]

So Leeds is right in identifying the claim that more virtuous theories are less likely to be true as an important part of the argument for constructive empiricism. It is this claim that underwrites the rejection of inference to the best explanation. For if explanatoriness detracts from the likelihood of a theory being true it should not be taken a reason to believe a theory.

However, as Leeds points out, it is only in comparisons of two theories where one is an extension of the other that the contention follows simply from probability theory. If we have two theories that each have content distinct from one another we cannot conclude from the fact that one is pragmatically more virtuous that it is less likely to be true than the other. Probability theory alone does not give us the tools to make this comparison. What van Fraassen needs is a argument that informationally virtuous scientific theories are in general unlikely to be true.

One argument might be that given there are so many theories that fit the evidence, when we limit the alternatives by applying criteria like simplicity we are breaking "the connection between degree of confirmation and probability of truth - unless we are prepared to make a priori assumptions about the simplicity and unity of the world..."[54] But such criteria are applied in our generation and selection of scientific theories. And since simplicity by itself counts as a virtue and is an essential part of what counts as a good explanation, we must conclude that the nature of our generation of scientific theories makes it unlikely that they are true. As Leeds sees it, what is implicit in this argument must be that whatever our knowledge of the chances of confirmationally virtuous theories being true, "...we do know that informationally virtuous theories have only a small chance of being true..."[55] What, then, of chances? We are presented with two alternative readings of the claim that informationally virtuous theories have only a small chance of being true: that the objective chance of virtuous theories being true is low or that our subjective probabilities of these theories being true is low. In both cases, Leeds argues, we have a claim that cannot be substantiated to the satisfaction of a realist; the claim is either false or amounts to question begging.

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[33] B. van Fraasssen, *Quantum Mechanics: An Empiricist View*, p. 3-4

First, consider the argument construed as one about a subjective probability, the likelihood we ought to assign to the truth of informationally virtuous theories independent of any claim about their objective probability. Its conclusion is that our subjective probabilities for informationally virtuous theories ought to be low. How could this claim be established? We could argue for it as follows: There are many theories that are empirically adequate and we know nothing about the theories that we consider to be informationally virtuous as regards to their truth value other than that they are members of this

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[53] B. van Fraasssen, *The Scientific Image*, p. 88

[54] S. Leeds, "Constructive Empiricism," p 200, quoting from P. Railton, "Explanation and Metaphysical Controversy."

[55] S. Leeds, "Constructive Empiricism," p.202

class. Hence we must treat them as random members of this class, of which most members are false. So, it must seem to us that these theories are unlikely to be true. The central question here is whether we ought to let informational virtues change our assignment of probabilities. Leeds's examines the argument by considering the consequences of accepting the principle he sees at work. Reichenbach's Principle says that all probability assignments must be based exclusively on the known proportions of target classes in reference classes. The examination turns on another principle that is necessary for use of Reichenbach's Principle to establish any inductive inferences, namely, that extra information about an object a, when the bearing of this information on the relevant proportions is unknown, cannot upset the probability we assigned to '*Fa*' before we got the extra information.

This principle is clearly at work in van Fraassen's argument but, Leeds claims, does not actually support the constructive empiricist contention, instead undermining it. He argues as follows: Most of the virtuous theories that we have sampled in the past have turned out to be true. So we should assign a high probability to any new virtuous theories. Further, by Reichenbach's Principle, we should not alter this assignment after we have learned that a theory involves unobservables. This is extra information about the theory and we do not know its bearing on the relevant proportions in the reference classes. Whether being a virtuous theory involving unobservables shifts the proportions in favor of truth or against it is not known. So the probability we assign to virtuous theories involving unobservables ought to be just as high as that assigned to virtuous theories in general, one that ought, indeed, to be quite high. It is thus the case that we can be justified in believing the things that theories tell us about the world behind the phenomena, not just what they tell us about observables.

Notice that this argument for belief in the non-empirical claims of theories bears striking resemblance to arguments for inference to the best explanation. We have a parallel between the sort of theory being talked about here and the theory about the mouse I looked at earlier. Having already discussed the problem with those arguments, I think this problem extends to Leeds' argument about the likelihood of virtuous theories being true. His hypothesis that informationally virtuous theories have mostly turned out true in the past is one about theories only involving observables, as he specifically stipulates in order to avoid begging the question against constructive empiricism. It follows that this hypothesis is equivalent to the hypothesis that most sampled informationally virtuous theories have turned out to be empirically adequate. The evidence cannot decide for us between the two. Should we then accept the stronger hypothesis and apply it as Leeds does to theories for which truth and empirical adequacy do not coincide? I don't think so. When Leeds says that the

> apparent flaw in this argument - the conclusion that we should assign a high probability to the next virtuous theory's being true, even after we learn that the theory involves unobservables - is of course no flaw at all: it is just another case of ignoring information whose bearing on the case at hand is unknown to us

he is making a mistake. There is something we know about the bearing of the information on the case at hand. An empirically adequate theory that only involves observables is a member of a much smaller class of theories than an empirically adequate theory about the same empirical facts that also involves unobservables. We can see this if we just imagine Leed's class of virtuous empirically adequate theories involving only observables. For each of these there are, I claim, many extensions that are equally virtuous and empirically adequate but also involve unobservables. Since the former class is finite (the class of past virtuous theories), the latter class must be larger than the former. Hence when we know that a theory's involving unobservables has negative bearing on the case at hand. A random member of a larger class ought to be assigned lower probability than a random member of a smaller class is assigned. So the fact that a theory involves unobservables tells us it is less likely to be true than if it didn't. We should not assign a high probability to the next virtuous theory's being true after we find out that it involves unobservables. Leeds has not adequately argued against constructive empiricism here.

However, Leeds favors the objective probability interpretation of the claim about virtuous theories having a small chance of being true.[56] He ventures three ways of establishing a low objective probability for informationally virtuous theories. Two of these involve considerations about our criteria and methodology in generating and choosing scientific theories. First, perhaps the chance our particular methodology is one which will lead us to true theories is quite low. Our standards for judging informational virtues are after all just one set among many and there is not any good reason to believe that they will lead us to the truth. Leeds thinks this argument begs the question. To see how we, must keep in mind this is a claim about how our actual standards of simplicity, explanatory power and so on match up with the world. To just flat out claim that they don't without some argument about why the world in fact doesn't fit with our criteria is to beg the question against the scientific realist. The second argument is intended to avoid this rebuttal. It says that we are committed to criteria, whatever their particular character, that are just one set among many possible and hence we are unlikely to have ended up with those that are likely to get us to the truth. This argument does not immediately beg the question against the realist. Leeds's reply? Well, dig a little deeper and you will see that again the constructive empiricist begs the question. For we have not, according to the scientific realist story, chosen our methodologies and criteria at random. We have been guided by nature in some way to those that are in fact likely to get us to the truth.

This 'guiding' is the result of "familiar laws and contingent circumstances."[57] The core of the counter-argument seems to be that our good chance of getting to the truth follows from the fact that "some, although by no means all, of the most fundamental laws of nature involve processes which appear also on the macroscopic level."[58] This 'fact' shows us that creatures such as ourselves, being able to figure out the macroscopic world, must have the right standards by which to judge explanation, not just of macro- but also micro-phenomena. This argument depends on already accepting the scientific realist conclusion - that our current theories have got things at the microscopic level right, give or take some details. Otherwise what justifies believing that there are microprocesses that mirror those we see at the macro level? The point on which this argument turns is the claim that even though there may well be a myriad of standards by which to judge explanatory adequacy, we have not chosen from these at random, but with some sort of 'guidance' from nature. But without the question-begging evidence that Leeds has presented, what reason do we have to believe this?

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[56] S. Leeds, "Constructive Empiricism," p. 201

[57] S. Leeds, "Constructive Empiricism," p. 203

[58] S. Leeds, "Constructive Empiricism," p.203

The third way to establish the low objective probability of informationally virtuous theories is taken from *Laws and Symmetry*. In a discussion of inference to the best explanation, van Fraassen here argues that because there are many theories that are as good explanations of the evidence as the best that we have now we ought to assign a low probability to the truth of our currently best theory. Against this claim, Leeds's asserts that in fact we have good reason to believe that there are not many such virtuous theories. His argument is that it really isn't all that convincing to claim that there are alternative theories that explain, for instance, what we see through microscopes. "Take for example," he asks us, "the hypothesis that what we see when we look at pond water through a microscope are actual little animals."[59] It is implausible he asserts to "suppose that there is some other hypothesis H which, if only the 17th century microscopists had thought of it first, would now play exactly the same role that the animalcule hypothesis now plays in our thinking."[60]

But why is this so hard to believe? The plausibility of the animalcule hypothesis rests in great part on the acceptance of a lot of physical theory about optics and the way that microscopes work. The achievement of a clear and unambiguous images from a microscope is outcome a of long process of skill acquisition, and largely informed by the prior acceptance of the veracity of the images that are striven for. There is every reason to expect that if our beliefs about any of those things changed enough we would explain the same evidence with a different theory. And our beliefs could be different in many ways. There is a multitude of regularities in the images produced by microscopes that are explained as artifacts of the process of microscopy. Why should we believe that it is

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[59] S. Leeds, "Constructive Empiricism," p. 204
[60] S. Leeds, "Constructive Empiricism," p. 205

impossible for many of the regularities in those images that we now take as accurately representing features of the little animals to be taken to themselves be artifacts of the process? If we had different beliefs about how microscopes work (or fail to work) or held different theories about light, this would certainly change what counted as a true image. These true images would probably suggest two radically different hypothesis about the subject of the imaging. It seems quite plausible when approached this way to suppose that there are a large number of equally explanatory theories that we may have never formulated.

However, constructive empiricism probably doesn't need to make this particular case. Leeds asks for an alternative hypothesis that plays exactly the same role in our thought as the animacule hypothesis. But the animacule hypothesis is too particular to decide the dispute here. That what counts as successful imaging in microscopy is embedded in a theoretical context suggests that we could not have an alternative hypothesis playing *exactly* the same role as the animacule hypothesis. If the animacule hypothesis were changed, a lot of other adjustments would be necessary. The exact same set of facts would therefore not be explained by such a hypothesis, since what counted as a fact would be different. The very particularity of Leeds's example is what makes it look convincing.

Further, Leeds maintains that in the case of scientific theories our inability to frame alternatives ought to be taken as good evidence that there are no such alternatives. This argument contains a suppressed appeal to inference to the best explanation - the best explanation of our failure to frame alternatives being that there really are none. While this may be an explanation, it doesn't strike me as the best one. The failure of our attempts to come up with even one really viable alternative on the spot should taken as evidence of the immensely complicated and evidentially intertwined set of theories that now make up science and our way of talking about the world, rather than as evidence that there are not any such alternatives.

I have argued that Leeds is not successful in undermining van Fraassen's contention that virtuous theories are unlikely to be true. Such attempts are bound I think to founder on the issue of inference to the best explanation. And here there does not seem to be enough common ground to decide the issue between constructiv empiricism and scientific realism. But there is still the more serious challenge, the one to constructive empiricism's coherence. In addition to Leeds, the issue of coherence has been raised or implied by Paul Horwich, Michael Friedman, Mark Wilson, Alan Musgrave, and Jeff Foss.[61] The issue appears also to be in the hovering behind concerns raised about van Fraassen's use of the observable/unobservable distinction to delimit the boundaries of epistemically justified belief.[62]

Leeds argues that constructive empiricism cannot consistently combine "the only reasonable account possible of observational adequacy", an internalist one, with the claim that theory acceptance need not involve belief in more than the empirical adequacy of the theory.[63] This is because internalist accounts of observability involve the claim that there is no theory-independent observation language. And this makes specifying empirical content a touchy business for constructive empiricism. Things look bleak, if Leeds is right about the impossibility of coherently maintaining internalism with skepticism about unobservables. The prospects for an externalist account of observability are not worth betting on; there are very persuasive arguments for internalism. So, given a conflict between internalism and constructive empiricism, the reasonable thing to do would be to reject constructive empiricism. But first let's see if Leeds is right that we have to choose between them.

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[61] M. Freidman, "Review of Bas Van Fraassen's *The Scientific Image*," S. Leeds, "Constructive Empiricism," and M. Wilson, "What Can Theory Tell Us About Observation?" A. Musgrave, "Realism Vs. Constructive Empiricism," S. Mitchell, "Constructive Empiricism and Anti-Realism," P. Horwich, "On the Nature and Norms of Theoretical Commitment."

[62] For instance, the arguments in P. Churchland, "The Anti-Realist Epistemology of Van Fraassen's '*The Scientific Image*,'" V. Harcastle, "The Image of Observables," and J. Foss, "On Accepting Van Fraassen's Image of Science."

[63] S. Leeds, "Constructive Empiricism", p.198

Constructive empiricism claims that science aims for empirically adequate theories, not wholly true theories. But maintaining that science has such an aim means having to distinguish between belief-that-a-theory-is-true from belief-that-a-theory-is-empirically-adequate. This requires specifying what the empirical content of a theory is, if we are to be able to say in any case what it is we are believing when we have a belief that the theory is empirically adequate rather than that it is true. Specifying this requires distinguishing what a theory says about observables from what it say about unobservables. And here van Fraassen's internalist account of observability causes problems.

It is clear that van Fraassen advocates internalism; we find it expressed in a number of places. For instance:

> [W]e cannot interpret science, and isolate its empirical content, by saying that our language is divided into two parts... The phenomena are saved when they are exhibited as fragments of a larger unity. For that very reason it would be strange if scientific theories described the phenomena, the observable part, in different terms from the rest of the world they describe.[44]

And

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[44] B. van Fraassen, *The Scientific Image*, p. 56

> To delineate what is observable, however, we must look to science - and possibly to that same theory - for that is also an empirical question.[49]

So there is no wiggle room here for van Fraassen, even if rejecting internalism were an attractive option.

Internalism has two elements: the claim that what is observable is an empirical matter to be decided by science and the further claim that there is no theory independent observation vocabulary. This kind of account of observability clearly entails that the language which spells out the empirical content of a theory is bound to be theory-laden. And theory-laden language, the coherence challenge asserts, must entail sentences making existential claims about unobservable entities. So the truth of the empirical claims of a theory entails the truth of (some of) the theory's claims about unobservables. The problem for constructive empiricism is that specifying what it is we are to believe when we merely accept a theory commits us to the truth of statements which entail others that go beyond what we are supposed to believe. This conclusion is argued to reveal the incoherence constructive empiricism's picture of science. One cannot consistently claim that the right attitude to take towards successful theories is belief that they are merely empirically adequate and also maintain a view of observability that forces you to believe that accepted theories are true beyond empirical adequacy.

Leeds suggests that there is only one viable escape for constructive empiricism: introduce a notion of the observational content of sentences that can isolate them from their non-observational entailments. If he can spell out what the observational content of sentences and thus theories is in such a way, then the constructive empiricist can urge belief only in what the theory says about the phenomena without the threat of incoherence. But Leeds argues that there is no such notion of observational content available to constructive empiricists.

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[49] B. van Fraassen, *The Scientific Image*, p. 57

Leeds argument on this point proceeds as if theories were sets of sentences, rather than families of models. This contributes to its ultimate failure to persuade, I think, but Leeds does present reasons to believe that going forward in this way doesn't make any difference in the end. So for the moment I will address the argument in the form Leeds presents it in, and temporarily leave aside the issue of the semantic nature of theories.

How then can we spell out the observational content of a theory? A theory's observational content is specified by first figuring out what it says about everything, observable or not. Only then do we go on to reckon which parts of what it says are about what is observable. Depending on the theory in question, this will be done using the resources it has to circumscribe the limits of measurement and observation and the resources provided by other theories we accept about instruments in use and ourselves as perceivers. How will we do this? Take a very simple case. Suppose we have a theory which entails a sentence of the form *Fa* where *a* names some observable entity and *F* names some observable property that *a* has. How do we specify the observational content of *Fa*? We look at what *F* denotes and what *a* denotes. But

> ...deciding what an 'observational' '*F*' denotes will typically depend on being able to say what at least some of the other terms in the language of T stand for - and typically not all of these terms will denote observables.[44]

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[44] S. Leeds, "Constructive Empiricism," p.191

That is to say,

> ...one cannot decide what belongs to the extension of an observation predicate unless one decides what the relevant theoretical terms denote.[67]

There will be situations where we cannot decide whether or not something falls into the extension of an observational predicate unless we can fix the extensions of (some of) the theoretical predicates, some of which do not denote observables.[68] Now the problem is that a constructive empiricist cannot in general indicate which situations are in the extension of an observational predicate, since he can't fix the extensions of unobservable predicates. Or so Leeds argues.

One option that Leeds explores consists in the constructive empiricist identifying those "actual situations in which people who believe T use the various predicates of T" as fixing the extension of observational predicates.[69] This falls short of a solution because what counts as observable, even for a constructive empiricist, goes beyond the situations in which anyone will be able to say, "Here is a G." But this is mistaken. Leeds has confined the range of situations to actual situations in which a realist is willing to say "This is a G." But it is unclear why this strict a restriction is necessary. We cannot take all those situations of which a realist is willing to say "That is a G" for they will include phenomena that constructive empiricism does not want to concede are observable. What is required is a determinate way to project beyond the actual situations in which an ordinary person who believes the theory will say "That is a G" without going beyond what is acceptable observable. Leeds believes there is no non-arbitrary way of doing this. But he is wrong. The non-arbitrary way in which constructive empiricists can project beyond the actual situations is by looking at what the relevant theories say. That is, we ask what phenomena would be observable if the theory were true, and that will determine its observational content.

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[67] S. Leeds, "Constructive Empiricism," p.194

[68] Indeed there will be cases where we cannot even do this even with the full power of the extensions of unobservable terms available to us. So determining the observational content of a theory is a problem also for the scientific realist. Its acuteness is markedly lower for the realist, however - he does not have the same need to uniquely specify the observational content of a theory that the constructive empiricist does. If you are not committed to being agnostic about whether or not the unobservable terms of a theory denote, then not being able to figure out the denotations of observation terms does not pose for you the same kind of serious problem. Leeds makes this point ("Constructive Empiricism," p. 196)

[69] S. Leeds, "Constructive Empiricism," p. 195

According to Leeds, this only gives a well-defined notion of observational content, not an absolutely determinate answer to the question of what the empirical content of a theory is. Hence he argues that as a strategy it will not work; the constructive empiricist needs more than simply a well-defined notion of observational content. Further, "identifying the observational extension of a predicate with the situations that would have been in the extension of that predicate, had the theory T been true" will not work. This is because "there will be ever so many ways in which it might have been true: in some of them objects resembling the stone I see before me will be lattices of carbon atoms, in others not." And so the counterfactual is not well defined, and we are not able to pick out the observational content of theory T. However, using the counterfactual construction here is incorrect. Constructive empiricism does not say that observationally adequate theories *are* radically false. It says theories might be radically false even if they are observationally adequate. We do not have to presume that a theory is radically false. So what we ought to use is a simple conditional: If the theory is true then what is its observational content? And answering this question gives more than just a well-defined notion of observational content.

It should also be emphasized that scientific realists do not deny that well-confirmed scientific theories might be radically false. They just deny that this implies that we are not justified in believing what they say about the world beyond the phenomena. And so whatever indeterminacy about observational content that the possibility of radical falsity introduces for a theory is there for them to deal with as well.

Part of the reason Leeds comes to the conclusion he does is that he has detoured around the semantic theory of theories. But constructive empiricists will not see their project in these terms. On the semantic view of theories, the linguistic expression of a theory specifies a family of models. Models intervene between sentence and the world.

> Scientific models may, without detriment to their function, contain much which corresponds to no elements of reality at all. The part of the model which represents reality includes the representation of actual observable phenomena, and *perhaps* something more... The idea is that the interpretation of language is not simply an association of real denota with grammatical expressions. Instead the interpretation proceeds in two steps. First, certain expressions are assigned values in the family of models and their logical relations derive from relations among those values. Next, reference or denotation is gained indirectly because certain parts of the model *may* correspond to elements in reality.[19]

Understanding theories this way allows the full use of the linguistic resources of a theory without realism about every entity it has a name for. Specifying the extension of an observation predicate can be done without any direct reference to the world. When we go about determining what the empirical content of a theory is, we have to consider what the theory itself says about everything and also what the relevant theories say is observable. From the results of these two projects an accounting of the empirical content of a theory can be given. All our talk is about models. If it reaches through them to the world, and reference and denotation is achieved it is because parts of the model correspond to parts of the world.

It might be thought that this suggestion merely pushes the problem back, not really solving it. Leeds, for instance, briefly argues that the semantic approach does not shed new light on the problem, "it merely locates them in a different place."[71] He maintains that when we say which of the substructures in a model are isomorphic to the world we will only be able to do this in theory laden language and that this language will have entailments about unobservables the constructive empiricist cannot embrace. So we will still have sentences with references to unobservables entailed when we specify the empirical substructures of our models.

When we assert that the claim that there are 10^{23} molecules of H_2O in the glass in front of us is observationally adequate what we are really claiming is that there is a theory, a family of models, that has substructures isomorphic to the glass, to what it contains, and to all the observations that can be made of them. We are no doubt letting our language be guided by a certain picture of what the world is like, by the model, but any entailments the claim has about unobservables are really about elements in the model not about the world. All the truth we need to specify the empirical content of a theory is truth according to the family of models. We can agree that sentences about unobservables are true in this way without conceding that they are true of the world.

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[70] B. van Fraassen, *Laws and Symmetry*, p. 213-14
[71] S. Leeds, "Constructive Empiricism," p. 199

An obvious objection is that this way of construing talk about unobservables is that it entangles us in talk of abstract objects that is equally (or more) unacceptable to empiricism. At this point I cannot fully answer this objection. Briefly, the answer is that we do not need to be realists about abstract objects: they are fictions. But this is a position for which I am going to have to argue. The details of how fictionalism about abstract mathematical objects can be maintained in conjunction with constructive empiricism, as well as what this fictionalism amounts to are issues with which the rest of this dissertation deals. I will return explicitly to the question of coherence for constructive empiricism in the final chapter.

At this point, let me sum up what I take to be the central features of constructive empiricism, those that characterize it and those that are relevant to developing a constructive empiricist philosophy of mathematics. Most generally, constructive empiricism is characterized by a rejection of knowledge claims about anything that goes beyond any possible experience, coupled with a refusal to give up the very linguistic resources that seem to entail acceptance of at least some of those same knowledge claims. Minimally, any constructive empiricist philosophy of mathematics must allow the full use of the linguistic resources of mathematical theories, without thereby entailing realism about every entity those theories have a name for. If constructive empiricism countenances mathematical objects they must be observable, whatever else they are. But such a philosophy of mathematics is probably going to have to be fictionalist. And given the move that van Fraassen makes to pragmatics—by distinguishing belief and acceptance—in order to undercut inference to the best explanation, the philosophy probably will involve pragmatic notions in an analogous way to avoid an inference to the existence of abstract mathematical entities. We find van

Fraassen making the following claim about constructive empiricism and mathematics:

> I do not really believe in abstract entities, which includes mathematical ones. Yet I do not for a moment think that science should eschew the use of mathematics, nor that logicians should, nor philosophers of science. I have not worked out a nominalist philosophy of mathematics—my trying has not yet carried me that far. Yet I am clear that it would have to be a fictionalist account, legitimizing the use of mathematics and all its intratheoretic distinctions in the course of that use, unaffected by disbelief in the entities mathematical purport to be about. Within mathematics, the distinction between structure of different cardinalities and the nonisomorphism of real number continuum and natural number series are objective.[72]

My ultimate aim, then, is to provide just such an account. But for now it is time to turn more explicitly to the philosophy of mathematics.

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[72] B. van Fraassen, "Empiricism in Philosophy of Science," p. 283

Chapter Two

Mathematics and Empiricism

This chapter rehearses some of the debates about mathematics in the philosophical literature. It is not intended as a thorough assessment or history of that literature, but as a look at empiricism and the philosophy of mathematics. I explore realist thinking about mathematics—both the positive accounts proffered and arguments against the possibility of a plausible empiricist account of mathematics. Realist reasoning is rejected, both on general grounds and on certain grounds specific to constructive empiricism, the kind of grounds that we saw in chapter one for rejecting scientific realism. The rejected views include the realism advocated by otherwise ontologically restrained and epistemologically empiricist philosophers like Quine. In this case, rejection is based on the structural parallel between the indispensibility argument motivating the mathematical realism and inference to the best explanation arguments in the philosophy of science that are repudiated by constructive empiricism. Quine's argument that "ordinary interpreted scientific discourse is as irredeemably committed to abstract objects—to nations, species, numbers, functions, sets—as it is to apples and other bodies" and that the "numbers and functions contribute just as genuinely to physical theory as do hypothetical particles" are rejected in constructive empiricist fashion, along with inference to the best explanation.[72]

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[72] W.V.O. Quine, "Success and the Limits of Mathematization," pp. 149-150.

I also consider the fortunes of anti-realist theories of mathematics, judging them again by both general and specifically constructive empiricist criteria. For empiricism, mathematics arguably poses the most difficult of its problems. Much of the philosophy of mathematics done in the last century stems from concern with the foundations of mathematics. The main competing philosophies of mathematics of the first half of the twentieth century—logicism, formalism, and intuitionism—all address concerns raised by the feeling that there is a crisis in the foundations of mathematics. But these concerns are different from what I take to be the main problem that mathematics raises for empiricism. I refer here to reconciling empiricist epistemology with the apparent truth of mathematical sentences.[73] This reconciliation seems to require violating empiricist scruples by allowing knowledge, possibly *certain* knowledge, of objects outside any possible perceptual experience. But the alternative is evidently just as unpalatable. A rejection of mathematical objects appears to require a rejection of mathematical truth and knowledge. One of the main tasks of this dissertation is showing how that appearance is at least partly misleading.

Both logicism and formalism present mathematics in a way that promises to solve the semantic problem it raises for empiricism. Both render mathematical truth innocuous by interpreting its subject matter away in one way or another. But neither philosophy is acceptable. There are general problems with both, and constructive empiricism cannot accept them precisely because of they try to *explain away* the semantic problem. They have elements that ring true, however, and that I aim to carry over into the account I develop—Carnap's distinction between internal and external questions, and the formalist recognition of a game playing dimension of mathematics, for instance. But these find expression differently and in different aspects of my account than in the theories from which they originate.

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[73] P. Benacerraf discusses this problem in "Mathematical Truth."

The major anti-realist philosophy of mathematics from which my account borrows is Kitcher's constructive naturalism. [74] This comes about because Kitcher's basic empiricist orientation and anti-realism about mathematics are compatible with constructive empiricism. The mathematical objects of Kitcher's theory, such as they are, are observable entities. They are the kind of entity that constructive empiricism allows knowledge of. In general, Kitcher's constructive naturalism draws a line between mathematical statements that we can believe to be true and those that (according to constructive empiricism) we cannot. And the line is drawn where constructive empiricism requires it: at the limits of possible experience. This alone shows the suitability of Kitcher's theory to a constructive empiricist philosophy of mathematics. But it also reveals an aspect of Kitcher's account that is important for constructive empiricism—it points to a solution of the semantic problem that respects the semantic view of theories.

But it is important to realize that Kitcher's theory cannot be adopted wholesale by constructive empiricism. His adoption of a pragmatic theory of truth in reaction to the semantic problem prohibits this. An alternative development of Kitcher's basic position is, however, possible, and even suggested by some of his own comments. This involves treating mathematical theories as stories, only parts of which are true, and (most) mathematical objects as mere fictions. But this move is only sufficient to answer the semantic problem if there is an acceptable anti-realist account of fiction itself; without one, we would merely be substituting one objectionable kind of object for another. Thus calling mathematical objects fictions and mathematical theories stories demands a theory of fiction.[75] This will be provided in the next chapter. For now, on to mathematics.

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[74] P. Kitcher, *The Nature of Mathematical Knowledge*, "Mathematical Naturalism," and "Mathematical Progress."

Coping with the following tension is a fundamental difficulty faced by every philosophy of mathematics. On the one hand mathematics appears to be an area in which we can unproblematically and confidently say that we have knowledge. On the other, mathematics also appears to take as its subject matter a realm outside of space and time with which we can have no contact.[76] It is hard to reconcile these apparently uncontentious facts. In spelling out the content of our mathematical knowledge the natural thing to do is to use standard semantics. One result of this process is that our mathematical knowledge is knowledge of abstract objects without location in space-time. But then how can we come by any knowledge of them? Benacceraf suggests that there is a fundamental conflict to be found here. He claims that

> accounts of truth that treat mathematical and non-mathematical discourse in relevantly similar ways do so at the cost of leaving it unintelligible how we can have any mathematical knowledge whatsoever; whereas those which attribute to mathematical propositions the kinds of truth conditions we can clearly know to obtain, do so at the expense of failing to connect these conditions with any analysis of the sentences which shows how the assigned conditions are conditions of their truth.[77]

And this is because

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[75] It is with this theory of fiction that pragmatics enter into the account of mathematics, not with the theory of truth as in Kitcher's theory.

[76] James Brown gives a simple argument for this: "There are infinitely many numbers, but only a finite number of physical entities; so most mathematical entities must be non-physical. It would seem rather unlikely that, say the first *n* numbers are physial while from *n* + *1* on they are abstract. So, the reasonable conclusion is that all numbers, and indeed all mathematical objects are abstract." (*Philosophy of Mathematics: An Introduction to the World of Proofs and Pictures*, p. 13 of draft)

[77] P. Benacceraf, "Mathematical Truth," p. 662. The influence of this paper on subsequent philosophy of mathematics is discussed and Benacerraf's problem reconstructed by J. Burgess "Epistemology & Nominalism."

> two quite distinct types of concerns have separately motivated accounts of mathematical truth: (1) the concern for having a homogeneous semantical theory in which semantics for the propositions of mathematics parallel the semantics for the rest of the language, and (2) the concern that the account of mathematical truth mesh with a reasonable epistemology."[78]

Benacceraf distinguishes between accounts of mathematical truth that attribute the normal syntax and semantics to mathematical statements from accounts that try to state truth conditions on the basis of something other than the standard semantics. He calls the latter type of account combinatorial, including among them all conventionalist accounts, Hilbert's view, and any others that give formal derivability in some system as truth conditions. This kind of philosophy of mathematics is distinguished by deviation from an account of truth that accords with Tarski's definition of the truth predicate. Tarski's definition proceeds through the mechanisms of quantification and predication, defining truth in terms of reference and satisfaction. Combinatorial views of mathematical truth are unsatisfactory because they "avoid... the necessary route to an account of truth: through the subject matter of the propositions whose truth is being defined."[79] Benacerraf's complaint is that because the truth conditions specified do not work in this way, they do not capture what it is we mean by 'true'. Hence even though combinatorial accounts may satisfy a desire to have an acceptable epistemology of mathematics, they make it unclear that mathematical knowledge really is knowledge. We can tell if some mathematical statement is a deductive consequence of a particular set of axioms but this does not in itself tell us that what the statement says is true, only that it follows from the axioms.

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[78] P. Benacceraf, "Mathematical Truth," p.661

[79] P. Benacceraf, "Mathematical Truth," p. 678

On the other hand, realist accounts of mathematical truth avoid this problem entirely. By assigning truth conditions to mathematical statements in the standard way, realist views associate these truth conditions with the statements' content in the right way. Thus, there is no puzzle about why the truth conditions are to be taken as conditions of truth.

Platonism and Structuralism

Platonism is the paradigm example of just such a realist philosophy.[80] It understands mathematical knowledge to be descriptive and about abstract mathematical objects that do not belong to the physical world. This means that the platonist is free to apply standard semantics in the normal way to mathematical statements to determine their truth conditions. Abstract mathematical objects provide referents for singular terms and non-empty domains to satisfy existential statements. The resultant continuity between semantics for mathematical language and semantics for the rest of language provides a strong argument for realism about mathematical objects. Another route to an account like this is a comparison of the apprehension of mathematical truth to the perception of physical objects.[81] Taking this as the basic insight that leads to platonism in the philosophy of mathematics, Dummett declares that for platonism the "mathematician is therefore concerned ...with the correct description of a special realm of reality, comparable to the physical realms described by the geographer and the astronomer."[82] However, mathematical propositions are generally presented as deductive conclusions or the results of computations. These kinds of presentation are not typical of reports of perceptual knowledge. Except in the case of fundamental assumptions or axioms there really does not seem to be a parallel. For these, in each case, there is something "which cannot be incorporated into a definition or other form of specification: the assumption, namely, that there *exists* a structure satisfying the axioms."[83] Dummett isolates this assumption as analogous to observation in the perceptual case. Gödel's philosophy of mathematics provides an example of this view. He maintains that "despite their remoteness from sense experience, we do have a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true."[84]

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[80] I do not mean to make claims about Plato's view of mathematics, but the family of views that have in this century (and earlier) been identified as platonic, for instance Gödel's expressed in "What is Cantor's Continuum Problem?" and "Russell's Mathematical Logic."

[81] This is one ground that J. Brown gives for his platonism in *Philosophy of Mathematics: An Introduction to the World of Proofs and Pictures* and in "π in the Sky." The quasi-platonist views of P. Maddy *Realism in Mathematics*, "Perception and Mathematical Intuition," "Physicalistic Platonism," and D. Bigelow *The Reality of Numbers* are in line here as well, differing only by placing mathematical objects in the causal world of space and time.

A further point in favor of platonism stems from Gödel's incompleteness result. This result entails, among other things, that arithmetic is incompletely formalizable. It follows that we are unable to give a formally determinate characterization of the structures investigated by the basic mathematical theories. There are statements in these basic theories that are true in their intended model yet not formally derivable. So it looks like there exists a structure of which a whole bunch of things, including some things not formally derivable, are true. This means that the structure is something more than and independent of the formal system within which *only some* of the truths about the structure are derivable. For instance, when doing number theory "we have in mind one determinate structure which we intend to investigate...[But] we find that any formalism we give allows other models besides the intended one."[85] Because our formal systems do not allow all of the truths of their intended models to be derived they do not uniquely specify those models. Hence, incompleteness means that we cannot be sure that what others refer to as the standard model is isomorphic to the standard model we have in mind. But there is not in practice any real uncertainty about whether a particular object is a natural number, say, or not. Number theory has an intended subject matter - the intended model, its members and their relationships - and this subject matter is shown by incompleteness to go beyond what can be captured by any formal system. This supports the notion that what mathematics studies is more than just formal systems, but is in fact some realm of independently existing objects. In other words, this supports platonism.

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[82] M. Dummett, "Platonism," p. 202
[83] M. Dummett, "Platonism," p. 207
[84] K. Gödel, "What Is Cantor's Continuum Problem?" p. 271

However, things are not all easy for platonism.

> [I]n the absence of anything corresponding to observation (with its possibility of negative outcome), it seems difficult for an assertion of abstract existence to get any grip; it slides and finds no friction - in the tired Wittgensteinian phrase, we do not know what it would be like for there not to be any real numbers, for example."[86]

This complaint may seem unfair. After all, if there were no real numbers then one way in which things would be, would be that mathematics is false, understood as platonism understands it. And if mathematics is false, it is difficult to see how it could be so useful. Our scientific descriptions and explanations of the world essentially involve mathematics. The scientific image is unavoidably mathematical. Scientific theorizing and explaining proceed mathematically. Mathematical properties are constantly appealed to, for instance, in the explanations of physics. For example, why does a ball reach a particular point when thrown up in the air? Because the velocity of the ball at any instant on its journey up in the air is the vector sum of its upwards and downwards velocities.

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[86] M. Dummett, "Platonism," p. 209

> When the two velocities are equal the ball stops its upward course. The downward velocity is equal to a_*t, so the ball stops its upward flight when and only when $v_* = a_*t$. Plainly this explanation appeals to several mathematical properties of the balls velocity... Moreover this velocity itself, being a function, is a mathematical object."[87]

If the mathematical facts appealed to in this explanation were not really facts, if they were not true, how could we be explaining anything here? For this to be an explanation it must have true premises and presuppositions. If mathematics were false we would not be explaining here. Thus a platonist can point to something that would be different if there were no real numbers - our science would not, could not, work the way it does. But notice that constructive empiricism cannot accept this reasoning. It will have to construe such explanations in a way that doesn't require all their premises and presuppositions to be true.[88]

Platonism, however, also makes mathematics' usefulness a mystery, perhaps even more of a mystery. Why should descriptive knowledge of a realm of abstract objects be of any use in understanding the physical world? What is it about the properties of numbers that makes knowledge of them relevant to concrete physical objects and processes? It seems strange that the properties of and relationships between transcendent, abstract objects should be of any use in describing or explaining physical objects. This observation has led some

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[86] M. Dummett, "Platonism," p. 212

[87] M. Resnik, "Naturalized Epistemology and Platonist Ontology," p. 471. Resnik also argues that both the way that scientists use mathematics and that mathematics works shows that it is true, since if it were false these things could not be as they are. (*Mathematics as a Science of Patterns*. See especially chapter three.)

[88] Van Fraassen has a pragmatic theory of explanation that will do this job. His account of explanation does not entail that only explanations with true premises and presuppositions can explain. See *The Scientific Image*, especially chapter five.

realists away from platonism. Michael Resnik and Stewart Shapiro both defend versions of structuralism, as does Geoffrey Hellman.[89] The realism they embrace makes the needed connection between the abstract objects of mathematics and the physical world. Mathematics for a structuralist is about structures or patterns. What the platonist takes to be the objects of mathematics - numbers, sets, and what have you - are just positions in the structures that are the real objects of mathematics. Thus what makes mathematical knowledge of use in science is that physical objects can be instances of the patterns that mathematics studies. Our mathematical knowledge is in this way also knowledge of the world.

A different concern lends further plausibility to structuralist accounts of mathematics. Since there are competing and equally good reductions of numbers to sets, we have no way of deciding which reduction is the correct one. That is, we have no reason to think that 3 is [[[Ø]]] rather than [Ø, [Ø], [Ø, [Ø]]]. Further, "any system of objects, whether sets or not, that forms a recursive progression must be adequate" for arithmetical purposes.[90] This indicates that it is not the members of the progression as individuals that matter to arithmetic, but the structure that they exhibit together as the progression. Benacceraf has argued that therefore "numbers are not objects at all, because in giving the properties (that is, necessary and sufficient) of numbers you merely characterize an abstract structure - the distinction lies in the fact that the 'elements' of the structure have no properties other than those relating them to other 'elements' of the same structure."[91] Numbers cannot be individuated by any properties independent of those that they hold in virtue of the role they play in the structure they belong to. If this is right, then it is puzzling to say that arithmetic knowledge is knowledge of numbers and their properties. If we cannot say what a particular number is independent of the role that it plays in a particular kind of structure, it makes more sense to think of arithmetic knowledge as knowledge of those structures rather than of the numbers themselves.

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[89] See M. Resnik *Mathematics as a Science of Patterns*, "Mathematics As A Science of Patterns: Ontology and Reference," and "Mathematics as a Science of Patterns: Epistemology," S. Shapiro, *Philosophy of Mathematics: Structure and Ontology*, "Space, Number and Structure: A Tale of Two Debates," and "Mathematics and Reality," and G. Hellman, *Mathematics Without Numbers* and "Modal-Structural Mathematics"

[90] P. Benacerraf, "What numbers could not be," p. 290

But however successfully structuralists and platonists can vitiate concerns about their views' abilities to account for the usefulness of mathematics or the status of mathematical objects, there is still an epistemological problem for both of them. A gap exists between the epistemic subject and the mathematical objects these views posit. In both accounts mathematical objects are abstract. Platonistic mathematical objects, whether they are the traditional numbers or structures of some kind, are transcendent. It is hard to see how we could come to have any information about them. Structuralism does fare a little better - the structures that mathematics studies do not all have to be transcendent; they can be exemplified in the physical world. But even here, it is hard to see how we could come to have information about infinite structures or how we could get from knowledge of concrete instantiations of mathematical structures to knowledge of the structures themselves. Constructive empiricism cannot accept this view of mathematics, any more than it can accept a platonist view. They both entail that we have knowledge that goes beyond any possible experience. If we cannot observe mathematical objects, which we can't if they are what either the platonist or the structuralist say they are, then we cannot claim to have knowledge of them.

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[91] P. Benacerraf, "What numbers could not be," p. 291

We have seen a few of the motivations for adopting a realist philosophy of mathematics. Semantic homogeneity, our intuition that mathematics studies independent structures, and the desire to have truth conditions for mathematical statements that clearly are *truth* conditions are all important considerations. Any acceptable philosophy of mathematics ought to be able to either satisfy these motivations or have an explanation of why it doesn't need to satisfy them. Empiricists are unlikely to give the demand for a homogenous semantics precedence over epistemology. Constructive empiricism can't—it has already rejected this move in the case of unobservable physical entities, where more than semantic homogeneity is at stake and so it can hardly consistently do so in this case. If the two cannot be simultaneously satisfied, as Benacceraf implies, semantic homogeneity will take a back seat to empiricist epistemology. Empiricism claims that experience is the sole source of our knowledge. If we allow that independently existing abstract objects are the subject matter of mathematics, it is only by stretching the concept of experience to include something like a special faculty of intuition as one of its mediators that even the appearance of an empiricist epistemology can be maintained.[92] This, however, is a move redolent of rationalism. The allowance of abstract objects into ontology would in effect be a letting go of empiricism.

Accusation of psychologism or subjectivism are a danger for any anti-realist philosophy of mathematics. Sacrificing the semantic simplicity achieved by a platonic account of mathematical truth risks a view open to this charge. As

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[92] One thinks of Gödel in this connection. Not as an empiricist grappling with the problem of mathematical truth, but more generally. Dummett points to this kind of move in the following passage: "Sometimes, however, the platonistic picture is put to another use than this. It is held by some platonists that we possess an intuitive apprehension of certain mathematical structures, which guides our formulation of the axioms of the theories which describe them, but may not be, at any given time, fully embodied in those axioms." (M. Dummett, *Frege: Philosophy of Language*, p. 510)

Dummett has pointed out, "[o]ne application of the platonistic picture of a mathematical reality external to us is as a means of expressing the conviction that mathematical statements are determined as objectively either true or false, independent of our means of proving or disproving them, just as are statements about the physical universe, on a realist interpretation of those statements."[93] Alternative pictures of what it is that makes mathematical propositions true are going to have a more difficult time expressing this conviction. If what makes mathematical truths true is not some external reality, as in platonism, we are likely to end up with the truth values of mathematical statements depending on our abilities in some way. It could be argued that this undermines the objectivity of mathematics. The exact nature of this complaint will depend on the details of the anti-realist view in question. An account that links truth to provability will be open to the objection that the truth of mathematical statements should not be dependent on our ability to prove or disprove them. Constructivist accounts of mathematical objects will be open to a similar complaint: the nature of mathematical objects, the truths about them, ought not to depend on our abilities to construct and their limitations. This is a worry to be kept in mind in developing or assessing any empiricist view of mathematics.

Logicism and Formalism

Logicism and formalism both superficially look like good candidate philosophies of mathematics from the constructive empiricist point of view. They both give accounts of the truth of mathematical propositions designed to avoid the need for non-concrete mathematical objects, logicism by reducing mathematical truth to logical or analytic truth and formalism by limiting it to truths about concrete, surveyable mathematical symbols.[94] Ultimately, although for different reasons, both these views fail. A discussion of them, however, is useful because there are things that they both get right, and to illustrate how the difficulties mathematics presents to empiricism can play out.

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[93] M. Dummett, *Frege: Philosophy of Language*, p. 506

The non-spatio-temporal nature of mathematical objects is particularly troubling to empiricists. Being non-spatio-temporal, they cannot be objects of experience, and hence knowledge of them is not possible. But if mathematics is somehow knowledge of things that we can and do experience then another problem arises. How can mathematical knowledge, if it is empirical knowledge, attain the kind of certainty or necessity if appears to have? One answer that empiricists have suggested is that mathematical truths are truths of logic. Our knowledge of mathematics, on this view, is the same kind of knowledge we have of logic. It is in some sense just knowledge of our own language and the proper way to use terms. Ayer adopts a position of this kind; he holds that the truths of logic and mathematics are necessary, but he is concerned that the empiricism he develops makes it impossible to account for knowledge of necessary truths. Ayer believes that propositions with factual content cannot be necessary or certain. Necessary or certain propositions cannot be in doubt, hence "no propositions whose validity is subject to the test of actual experience can ever be logically certain."[95] In order to maintain his empiricism, then, Ayer must either reject the necessity of the truths of mathematics and logic or else he must conclude that they indeed have no factual content.

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[94] Russell and Whitehead's reduction of classical mathematics to a single formal system in *Principia Mathematica* attempts to realize the logicist hope of deriving all mathematics from pure logic without using any extra-logical assumptions. The logicist view of mathematics is also exemplified in A.J. Ayer "The A Priori" and C. Hempel's "On The Nature of Mathematical Truth." Expressions of formalism can be found in D. Hilbert, "On The Infinite," von Neumann "The Formalist Foundations of Mathematics" Michael Detlefsen is a recent defender of Hilbert in *Hilbert's Program*.

[95] A. J. Ayer, "The *A Priori*," p. 315

Ayer rejects the possibility that mathematical knowledge is contingent or factual on the grounds that there are no circumstances in which we would say that a mathematical statement had been shown wrong. No experiment that we could perform could have results that led us to reject any truth of mathematics. We would always retrench and say that we had measured wrong or something of the sort if an experiment had a result that appeared to contradict mathematics. Not being contingent or factual, mathematical knowledge must then be analytic.

Carl Hempel comes to a similar conclusion, arguing from the question of justification. He asks what it is that sanctions the acceptance of mathematics and, after rejecting self-evidence and induction as grounds, settles on analyticity. According to Hempel, the problem with induction as a ground for accepting mathematics is that it implies that we ought to be able to say what evidence would show some mathematical truth to in fact be a falsehood in a particular case. We cannot do this, and, moreover, it doesn't strike us as strange that we cannot. Any contender for falsifying evidence is and would be simply treated as evidence of mismeasurement somewhere, leaving the mathematical 'hypothesis' untouched. The self-evidence of mathematical truth is ruled out by the fact that results in some cases run counter to "deeply ingrained intuitions and the customary kind of feeling of self-evidence."[96] Indeed, there are some very elementary conjectures that are as yet undecided. The very fact of development in mathematics makes self-evidence a poor choice for an epistemology of mathematics. Our belief in some mathematical propositions has gone from (relatively) unjustified to (relatively) justified in the course of its development. This situation seems remarkable if self-evidence really were the way in which mathematical propositions acquired justification. It isn't the self-evidence of these propositions that has changed, but the availability of proofs or other appropriate mathematical evidence. Moreover, even if this were not the case, self-evidence is psychological and subjective not the sort of feature that should count for much epistemologically.

Hempel concludes that mathematical statements are analytic; the "validity of mathematics rests neither on its alleged self-evidential character nor on any empirical basis, but derives from the stipulations which determine the meaning of the mathematical concepts, and... the propositions of mathematics are therefore essentially 'true by definition'."[97] Ayer concurs. He argues that the truths of mathematics are necessary because they have no factual content. They are necessary because they are analytic. And analytic truths, for Ayer, are those truths whose truth depends solely on the definitions of the terms it contains. Hence mathematical truths are true as a result of definitions. Such truths, while not giving us any factual information about the world, do indicate something. They are illustrations: they show how we use certain symbols. Ayer's idea is that analytic truths "call attention to linguistic usages, of which we might otherwise not be conscious, and they reveal unsuspected implications in our assertions and beliefs."[98] And so, mathematical and logical truths 'simply record our determination to use words in a certain fashion."[99] Though I think that logicism fails, and don't want to endorse either Hempel's or Ayer's claims about analyticity, this idea has some

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[96] C. Hempel, "On The Nature of Mathematical Truth," p. 377

[97] C. Hempel, "On The Nature of Mathematical Truth," p. 378

[98] C. Hempel, "On The Nature of Mathematical Truth," p. 322

[99] C. Hempel, "On The Nature of Mathematical Truth," p. 326

merit. It is worth noticing that (at least some) mathematical truths, like truths of logic, reveal something of our use of language as much as they do about the external world. I do not want to make a lot out of this, but it is a point that will come up again in a slightly altered form in the constructive empiricist account of mathematics outlined in the last chapter.

Both Hempel and Ayer are using logicism to solve the problem of mathematical truth. Logicism claims that the truths of mathematics are just truths of logic. Hempel tells us,

> Mathematics is a branch of logic. It can be derived from logic in the following sense: a. All the concepts of mathematics, i.e. of arithmetic, algebra, and analysis, can be defined in terms of four concepts of pure logic. b. All the theorems of mathematics can be deduced from those definitions by means of the principles of logic (including the axioms of infinity and choice).[100]

On the surface this is a view that looks attractive to empiricism. Logicism can provide a neat answer to the empiricist problem of mathematical knowledge. Our knowledge of mathematics turns out to be knowledge of how we use certain concepts and of their logical consequences. Essentially, mathematical knowledge is knowledge of our own language, something we can be certain of. And given that it is logical knowledge, it is not knowledge that requires special objects.

However, this turns out to be mistaken. A few problems conspire to undermine the logicist foundational project and the view that mathematical truths are analytic or truths by convention. First, the reduction does not really obviate the need for special objects of knowledge. This is given away in the parentheses of Hempel's characterization of logicism. There we are told that the axiom of infinity is a necessary part of the reduction of mathematics to logic. And the axiom of infinity is an existential axiom. There need to be infinities in order to satisfy this axiom. Hence any model that makes mathematics true will have to have infinite domains. But we cannot count on concrete objects to be numerous enough to supply such domains. Thus, logicism fails, insofar as it is an attempt to provide an account of mathematics and mathematical truth that does not require the existence of special mathematical objects.

A graver problem, however, is highlighted by Quine's argument against the view that logical truths are true by convention.[101] The problem is that there are not a finite but an infinite number of logical truths. If convention or agreement generates these truths the process doing so could not get started. Because there are infinitely many truths of logic, we cannot characterize them one by one. Since this cannot be done one by one, it will have to be done via general principles. But we can't do *this* if we did not already have some logical principles at hand. The difficulty is that we could not generate the instances from the general principles without already having some logic to work with. But it is precisely this that is lacking, since we talking about the process of generating the truths of logic. Hence, convention or stipulation cannot have generated the truths of logic. But as I mentioned above there may still be something to the idea that there is a very close relation between the truths of mathematics and convention or stipulation. I will return to this in the final chapter. If we let go of the idea that mathematical propositions are true, then stipulation, agreement or convention can provide a criterion for dividing them up into two truth-like classes—those we accept and those we reject.

Getting back to logicism, even if truth by convention were not a problem, there is still the obstacle Benacceraf

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[100] C. Hempel, "On The Nature of Mathematical Truth," p. 378

[101] W.V.O. Quine, "Truth By Convention."

outlines. Granting that the truths of mathematics are true as the result of stipulations and conventions produces a combinatorial account of mathematical truth. The question is how stipulations and conventions can generate *truth*. We can stipulate all we like, have all the conventions in the world, but if the concepts involved do not refer, then the statements we make with them can not be true. Russell was wrong about the advantages of postulation. "For with theft at least you come away with the loot, whereas implicit definition, conventional postulation, and their cousins are incapable of bringing truth. They are not only morally but practically deficient as well."[102] Postulational stipulation is not up to the job of producing truth. So the logicist characterization of mathematical truth will not do.

And the same complaint against holds against formalism. Formalism might seem an attractive philosophy of mathematics to an empiricist since it too seeks to avoid the need for abstract objects. Crude formalism holds that mathematics is merely a game played with pencils and paper, chalk and chalkboards. Mathematical symbols are just marks, they are not about anything. If the truths of mathematics are just series of marks, some justified by the rules of the game, some not, then perhaps we can reconcile the conflicting demands of semantics and epistemology. But the 'truth' conditions this generates are again unacceptable on the grounds Benacceraf gives—it detaches semantics from truth. If truth is equated with formal derivability then the truth conditions given avoid "the necessary route to an account of truth: through the subject matter of the propositions whose truth is being defined."[103] Being formally derivable cannot guarantee truth. Truth can only be had by ensuring that what the mathematics says is the case, actually is the case. So this won't do as a characterization of mathematical truth.

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[102] P. Benacceraf, "Mathematical Truth", p. 679

[103] P. Benacceraf, "Mathematical Truth", p. 678

Consider, however, a more sophisticated formalism, Hilbert's view. For Hilbert only finite mathematics can be meaningful and true. Finite mathematical truths are about pencil marks on papers, but mathematics is not merely a game. The finite truths of mathematics are truths about the structure of our perceptual experience. Pencil marks on paper like ||| are objects of perception, and certain truths about them are evident. For instance, it is evident that the series || and || are put together they result in the series ||||. We write this truth: "2+2=4". All finite meaningful mathematics is for Hilbert of this kind. It is about mathematical symbols. Restricting the objects of mathematics to finite ones in this way alleviates the epistemological difficulty of saying how it is we can know anything about them. If, as Hilbert argues, mathematics is really only about finite objects then it does not have to be about abstract objects. This is clearly what Hilbert has in mind. "The subject matter of mathematics is... the concrete symbols themselves whose structure is immediately clear and recognizable."[104] But this leaves all the rest of mathematics, the stuff that clearly is about infinities. Hilbert must say something about its status. So part of his Program is to show that the mathematics that *seems* to be about infinities, while not strictly meaningful or true, is a legitimate tool for generating meaningful, true, finitistic results. Hence his goal of proving with finite techniques the consistency of mathematics involving infinities.

The concern is that that combinatorial accounts of mathematical truth do not give truth conditions that are really *truth* conditions. Hilbert's position, however, can be understood in a way that avoids this criticism. The semantics for finite mathematics are straightforward and referential. There are sufficient resources of concrete objects to satisfy the sentences of mathematics that are finitistic. However, because there are not and can not be an infinity of "the concrete symbols themselves," any existential statements about infinities are false. But this is just about what Hilbert believes in any case. His challenge is to show that the use of infinite mathematics will not introduce contradiction, to show that infinite mathematics is consistent.

Gödel's work on incompleteness undermined Hilbert's program.[105] But this is a different problem from the one Benacceraf suggests. If mathematics is taken to have two parts—one finite and true, the other infinite and false—it is possible for the semantics for mathematical statements to provide us with real truth conditions. Gödel's second theorem establishes that if a formal system sufficient to formalize arithmetic is consistent there cannot be a proof of its consistency that does not use more powerful mathematics. Since Hilbert's aim was to produce such a proof and there cannot be one, it is not possible to show to his satisfaction that infinities can be used as ideal elements. Use of infinite mathematics to reach finitistic results cannot be proofed against the introduction of falsehood. Indeed, Gödel's second theorem poses a deep problem for any mathematical anti-realist. Relative consistency for a set of axioms can be established by displaying a model in which the axioms are true. But for mathematical axioms of any interest a model that satisfies them will have to be infinite. The model will contain some objects, say the positive integers. If the model satisfies the axioms consistency will have been shown relative to the existence of

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[104] D. Hilbert, "On the Infinite," p. 192

[105] Or at least this is the standard position taken on the matter. Detlefsen in *Hilbert's Program* has argued the contrary. See especially pp. 77-141. Others also suggest that the defeat of Hilbert's

those objects. But infinite models are not to be had in the concrete world. These models are abstract mathematical entities themselves, and infinite ones. As such, they are not available to an anti-realist or finitist about mathematics. Hence, the anti-realist is not in a position to show even relative consistency.

But while it seems likely that Gödel's second theorem does generate a fatal problem for Hilbert's project, and it introduces a difficult problem for empiricists into the job of accounting for mathematics, it should be pointed out that the position of realists is not that much better. Gödel establishes that for any system rich enough to express arithmetic, if it is consistent then there is no proof of its consistency. And this result makes any display of a model to show consistency really rather weak. Such a display as a technique for proving consistency raises the problem of infinity just as much as a proof based on the syntactic character of a mathematical theory. As a proof of consistency this is especially weak since we cannot really *display* the models in question. They are infinite! So the absence of a proof of consistency is problematic for everyone.

I will be returning to this point in the final chapter, since the philosophy of mathematics I outline is finitistic and has similarities to Hilbert's. The finitism of my account makes Gödel's result a particular concern, but I will be arguing that it is not nearly so serious as it might appear, since along with finitism the account embraces the contingence of mathematical knowledge. A proof of consistency would be nice, but is not a necessity. Another part of formalism that reappears in my account is the notion of mathematics as game-playing. It doesn't emerge as it does in the stereotype of

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program is not so clear or complete. See J. Hintikka "Hilbert Vindicated," J. Webb *Mechanism, Mentalism and Metamathematics*. I will be returning to this point in the final chapter.

formalism. Mathematics is not a game of merely manipulating meaningless symbols. But the notion of a game plays a central role nonetheless.

Empiricist Realism and Indispensability

Perhaps the failure of logicism and formalism should lead us to reconsider the empiricist avoidance of abstract objects. Indeed, this is something that both Carnap and Quine have done. In "Empiricism, Semantics and Ontology," Carnap argues that most disputes about the existence of abstract objects are founded on a confusion. He intends to show that adopting and using a language that refers to abstract objects "does not imply embracing a Platonic ontology but is perfectly compatible with empiricism and strictly scientific thinking."[106] To establish this, he first distinguishes between internal and external questions. When we wish to speak about a new kind of entity we introduce a linguistic framework for the new entities. "The acceptance of a new kind of entities is represented in the language by the introduction of a framework of new forms of expressions to be used according to a new set of rules."[107] Questions relating to a framework may be internal or external; the question about the existence of some entity or class of entities is, he asserts, a question internal to a framework. Those concerning the existence of the system of entities as a whole are external questions.

Using the example of 'thing language' Carnap makes the following points: Internal questions can be raised and answered by empirical investigation. The concept of reality occurring in internal questions like "Are unicorns real?" and "Is there a book in the kitchen?" is an "empirical, scientific, non-metaphysical concept."[108] However, the external question about the reality of the thing world itself is of a different nature. Thought to be a theoretical question asking about the application of the term 'real' to the system itself, it is framed wrongly. Such an application of 'real' can only occur within a framework with its set of rules about the application of the term 'real'. The question of the reality of the thing world itself can only sensibly be a practical one about the desirability of adopting the forms of expression in the framework of things, not one about the application of the term 'real'. We can choose to adopt or reject the thing language, but the framework itself does not provide the resources to decide about the reality of 'things'. Frameworks only provide resources to decide about the reality of kinds of things or particular things.

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[106] R. Carnap, "Empiricism, Semantics, and Ontology," p. 242

[107] R. Carnap, "Empiricism, Semantics, and Ontology," p. 249

The reality of abstract objects is similarly a question of the external sort. Carnap claims that philosophers writing about the question have not succeeded in giving it any "cognitive content."[109] Against empiricists and nominalists who reject abstract objects on the ground that we have no warrant to believe in them, Carnap argues that this objection is based on the confusion of an external question for an internal one. To object that we have no reason to believe that abstract objects are real is to be confused about the application of the term real. The demand for theoretical justification is misplaced in this context. What is being asked for can not be empirical evidence but must really be the reasons for accepting a way of talking that includes abstract objects. I may, for example, legitimately question the existence of imaginary numbers from within the framework of mathematical language. The framework of mathematical language will give criteria for determining whether imaginary numbers are real. But I may not question the existence of mathematical objects in general. The framework does not provide the resources to answer this question.

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[108] R. Carnap, "Empiricism, Semantics, and Ontology," p. 243

[109] R. Carnap, "Empiricism, Semantics, and Ontology," p. 245. Carnap refers to Paul Bernays' "On Platonism in Mathmatics" as an example of the view that platonism is the result of admitting variables of abstract types. Bernays characterizes platonism in the following way. "...the tendency of which we are speaking consists in viewing the objects as cut off from all links with the reflecting subject. Since this tendency asserted itself especially in the philosophy of Plato, allow me to call it 'platonism.'" ("On Platonism in Mathematics," p. 259) He does not make the kind of internal/external question that Carnap uses to try and avoid the problem of platonism.

Carnap argues that the nominalist or empiricist view that belief in abstract objects is akin to superstition or myth falls afoul of the same confusion. Myths or superstitions are "false (or dubious) internal statement[s]"[110] whereas belief in abstract objects is merely the acceptance of a certain way of talking. "Generally speaking, if someone accepts a framework for a certain kind of entities, then he is bound to admit the entities as possible designata. Thus the question of the admissibility of entities of a certain type of abstract entities in general as designata is reduced to the question of the acceptability of the linguistic framework for those entities."[111] The decision to accept a framework is, then, not epistemic, but practical. Searching for epistemic justification is misguided.

His argument entails that an epistemological objection to abstract mathematical objects like Benacerraf's is mistaken. Our very acceptance of a certain way of talking—one that includes reference to abstract objects like numbers—is all there is to the claim that there are abstract objects. To object that we could not possibly be in a position to know anything about them, to demand an explanation of how we could, is mistaken. Talking this way implies that the existence of abstract entities is an internal question that the facts can answer one way or another. However, our warrant to believe in abstract mathematical objects comes from the justification we have for adopting the framework of scientific language which includes mathematics. This justification is pragmatic and not epistemic. We are justified in using mathematical language not because we perceive real numbers but because using mathematical language helps us get around in the world better. To ask how we can know there are abstract mathematical objects is like asking how we can know that there are things, and is similarly mistaken.

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[110] R. Carnap, "Empiricism, Semantics, and Ontology," p. 254

[111] R. Carnap, "Empiricism, Semantics, and Ontology," p. 253

Carnap also tries to diffuse the empiricist concern about accepting abstract objects by claiming that the acceptance of a linguistic framework is not to be interpreted as the acceptance of a belief in the reality of the kind of entity it deals with. For example, the "acceptance of the thing language leads, on the basis of observations made, also to the acceptance, belief, and assertion of certain statements. But the thesis of the reality of the thing world cannot be among these statements, because it cannot be formulated in the thing language." If someone decides to use thing language it does not mean that he has accepted a "*belief* in the reality of the thing world; there is no such belief or assertion or assumption."[112] The corollary of this for mathematical objects would be, then, that our decision to use mathematical language does not mean that we have accepted a *belief* in the reality of the mathematical world; there is no such belief or assertion or assumption. So we need not worry ourselves with empiricist scruples about the ground for that belief.

However, to say that acceptance of a linguistic framework does not lead to a belief in the reality of the kind of thing that the framework refers to ducks the question. If we take standard semantics seriously then our commitment to the truth of statements referring to any particular mathematical object entails our belief that that object has reality. It is the existence of certain sets that satisfies the truth conditions for the statements of set theory. Saying that the belief in sets only amounts to adopting a linguistic framework does not take this point seriously enough. If sets are not real, they are not out there to make set theory true. Our merely adopting a way of talking will not make it true. So, in accepting the framework of mathematical language, either we are accepting that mathematical objects really exist, or else we have to accept that statements which existentially quantify over them, or otherwise refer to them, are false. Carnap's strategy for avoiding commitment to abstract objects while still holding onto the language that refers to them is, then, ultimately unsuccessful. The problem with the way that Carnap uses this distinction is that he wants at the same to hold onto the claim that internal claims can be genuinely true, and that truth requires something of the world, without agreeing that this entails belief in the existence of the thing that the true claims are about. However, the distinction between internal and external questions is a good one. We should be aware that there is a difference between asking from inside a theory, "Are there imaginary numbers?" and asking generally, critically, if there are any numbers. We can answer yes to the former question and no to the latter without inconsistency. I will return to this in the final chapter.

Arguing that we need not have evidence for our belief in abstract objects seems odd. If I am right that as Carnap understands it, the acceptance of mathematical language really does amount to a belief in the reality of mathematical objects, then surely we will want epistemic grounds for this acceptance. Our reasons for adopting a certain framework will be broadly speaking pragmatic. How can pragmatic reasons provide

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[133] R. Carnap, "Empiricism, Semantics, and Ontology." p. 243

epistemic justification? The claim that we should (indeed, do) believe in abstract objects follows from their necessity in satisfying sentences of mathematics that we take to be true; the semantics for the mathematical language that we have adopted requires that there be certain abstract objects to make certain sentences true. What is in question is whether or how we can know that those truth conditions are actually satisfied. However strong, no pragmatic justification for our adoption of a way of talking speaks to this issue, indeed, can speak to it. We may be justified in adopting mathematical language because it pleases us, makes it easier to do our banking or fix motorcycles, but no justification of this kind can give us reason to believe that mathematics is true. The justification just isn't epistemic.

Notice that the argument that belief in mathematical objects is justified by the usefulness of mathematical language is the same as the argument made by scientific realists about unobservables in scientific theories. Scientific realists argue that we have good reasons to accept scientific theories involving unobservables. The acceptance of these theories is instrumentally useful to us - it helps us build things for example. Further, theories involving unobservables are simpler, more powerful and easier to use. But remember the lesson of constructive empiricism. All pragmatic virtues can justify is the *acceptance* of the theories. So we cannot accept the reasoning here. The problem is related to Benacerraf's complaint about combinatorial accounts of mathematical truth. The reasons given are not in and of themselves reasons for belief in truth. Like formal derivability, usefulness does not by itself point to the satisfaction of truth conditions. Pragmatic justifications cannot give us reason to believe that we are in a position to know that the truth conditions of the theorems of ZFC are satisfied any more than pragmaic justification can give us reason to think that we are in a position to know that the truth conditions of the theoretical claims of quantum mechanics are satisfied.

Carnap's thinking on this matter is related to a kind of argument that has become central in recent philosophical arguments about mathematical existence. This is the indispensability argument. Contemporary versions of the indispensibility argument derive from Quine and Putnam.[113] Unusually, for an empiricist, Quine adopts a realist view of mathematical entities. I mentioned at the end of the last chapter this kind of argument for mathematical entities. They go something like this:

> The scientific theories that we have are by and large very successful. This couldn't be the case if they were not on the right track. So, a lot of their presuppositions and what they explicitly say must be true. Most of modern mathematics is used by, and, so, presupposed by scientific theories, particularly physical theories. Mathematics is, indeed, essential to science. Hence we must believe that whatever entities are posited by mathematics really exist. Mathematics is indispensable for science, and since mathematics posits the existence of certain abstract objects, we are required to believe that these exist.[114]

The thought here is that we ought not to reject the existence of mathematical objects a priori. As good naturalists, we must be more open to the pronouncements of science and less ready to engage in premature philosophical decisions about ontology. Our justification for believing in abstract objects is that science says there are abstract objects. And so much the worse for empiricist scruples developed without sufficient regard for science.

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[113] See, for example, H. Putnam "What Is Mathematical Truth," and "Philosophy of Logic" and W.V.O. Quine "On what there is."

[114] Putnam says that quantification over mathematical entities "is indispensable for science, both formal and physical; therefore we should accept such quantification; but this commits us to accepting the existence of the mathematical entities in question. This type of argument stems, of course, from Quine, who has for years stressed both the indispensibility of quantification over mathematical entities and the intellectual dishonesty of denying the existence of what one daily presupposes." (*Mathematics, Matter, and Method*, p. 347)

Notice, however, that the indispensability argument for mathematical entities depends on more than is apparent from the way I have expressed it. It depends on two claims about science itself. There is a presumption that scientific practice accords with indispensibility and, in addition, that confirmation is holistic. Insofar as the indispensability argument gets its force because of a background assumption of naturalism, the argument should conform to what we know about actual scientific practice. To fall short of this will undermine the naturalism that is presumed. Both these assumptions of indispensability are open to question.

Confirmation holism tells us that theories face experience as corporate entities, not in bits and pieces. Hence, the empirical success of a theory confirms the mathematical claims embedded in it. However, confirmation may not be holistic. Perhaps evidence favors theories relatively. Consider the following: a set of observations O favors T_1 over T_2 if and only if $P(O/T_1) > P(O/T_2)$. This entails that anything that is shared by T_1 and T_2 is not confirmed by O. What can we infer from this about the confirmation of mathematics through the confirmation of scientific theories? Well, if the same mathematical statements are a part of each theory that is tested, then the outcome of a test cannot favor the mathematical statements over any competitors.[113] And, indeed, this is generally the case. In other words, if we reject confirmation holism, we can see that the indispensability argument does not go through as it stands.

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[113] This line of argument is from Eliot Sober's "Mathematics and Indispensability." Charles Chihara and Charles Parsons have also objected to the assumptions about confirmation and the assumed relation between mathematics and science that appear to be a necessary part of the indispensibility argument. (C. Parsons "Quine on the Philosophy of Mathematics" and C. Chihara *Constructability and*

Mathematics is part of almost all scientific theories; the same mathematics is used by theories that are confirmed and those that are disconfirmed. Mathematics, then, does not get the same kind of testing as the empirical parts of theory. Also relevant is the fact that in the face of recalcitrant experience or data, we are unlikely to decide that the mathematics we are using is false. This is not to claim that there could be no data or experience that would lead us to this conclusion, but just to point out that our attitudes towards theories are not homogeneous, the relationships we take there to be between data and various parts of theories come in different strengths and kinds.

This brings me to the general question of how well the indispensability argument accords with scientific practice. The historical record shows that the attitudes towards different parts of theories vary a great deal. Penelope Maddy mentions late nineteenth century attitudes towards atomic theory.

> [T]hough atomic theory was well-confirmed by almost any philosopher's standard as early as 1860, some scientists remained skeptical until the turn of the century... and even the supporters of atoms felt this early skepticism to be scientifically justified, This is not to say that the skeptics necessarily recommended the removal of atoms from, say, chemical theory; they did, however, hold that only the directly verifiable consequences of atomic theory should be believed, what ever the explanatory power or the fruitfulness or the systemic advantages of thinking in terms of atoms.[116]

The example is important because it shows that in science empirical success is not always taken to confirm all of the statements of a theory. Things are not as simple as the indispensability argument would have it. Another element of scientific practice that does not fit well with indispensability is the way in which mathematical models in

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Mathematical Existence.) Michael Resnik responds with a defense of confirmational holism in his *Mathematics as a Science of Patterns*.

[116] P. Maddy, "Indispensability and Practice," p. 280-81

science idealize. Looking at any elementary physics textbook will verify this. You will see example after example of mathematical equations that are applied yet explicitly agreed to be untrue of the systems to which they are applied. Without the false assumption of such mathematics, physical theories could not be applied. So we have here a practice in which mathematics is taken to be indispensable, yet false. Not a happy thing for the indispensability theorist. There is room, then, to question indispensability. Its presumptions about science make it vulnerable. It seems to me that concerns about the presumptions that indispensibility arguments make about confirmation, however, are only sufficient to cast suspicion on the indispensability argument. But there are other grounds on which we should be more than just suspicious. This is the topic of the next section.

Empiricism and Mathematical Fictionalism

It is, as Benacceraf highlights, the *truth* of mathematics that generates an epistemological problem for philosophy of mathematics. Indispensability points to science for evidence that mathematics is true. Our scientific practices, it argues, require us to accept mathematics as true. The step from here to realism about mathematical objects is the one via standard semantics that we are familiar with. There are two main ways to subvert the indispensability argument. Both undermine its conclusion by denying that mathematics is true. One strategy argues that mathematics is not indispensable to science, the other that the indispensibility of mathematics to science does not require its truth. With the move from indispensability to truth blocked, the move from truth to realism can not be made.

Benacceraf locates a tension for philosophy of mathematics in the interplay of the demands of epistemology and the desire for semantic continuity. The tension is especially strong for constructive empiricism with its rejection, as epistemologically suspect, of all knowledge claims about unobservables. This tension, however, only follows given the assumption that mathematics is true. If the mathematical propositions that generate epistemological difficulty are false, we can easily reconcile standard semantics and epistemology. Standard semantics do not independently entail the existence of abstract mathematical objects, but, rather, only a conditional: If mathematics is true then there must be abstract mathematical objects. Further, saying that mathematics is false need not suggest that standard semantics are not the right ones for mathematical statements. Denying the truth of mathematics allows one to accept both the standards semantics and an acceptable epistemology. If mathematics is false, then there is not problematic knowledge of abstract objects to account for. The question is whether the truth of mathematics can be plausibly denied.

Hartry Field maintains that we can do just this, and focuses on the first strategy to undermine indispensability, arguing for dispensability. Of the relation between mathematics and science, Field says that "by focusing on the question of application, I was led to a surprising result: that to explain even very complex applications of mathematics to the physical world (for instance, the use of differential equations in the axiomatization of physics) it is not necessary to assume that the mathematics that is applied is true, it is necessary to assume little more than that mathematics is consistent."[117] Field's project seems to take at face value the claim that if mathematics is indispensable then it must be true, and that the standard semantic account of truth ought to be accepted for mathematical language. But he is unwilling to believe that there are abstract objects. So he concludes that mathematics is false. His idea is that he can account for the usefulness of mathematics even though it is false by showing that it is not essential. It is useful simply because it provides a shortcut. Field's strategy, then, is to show that "mathematical entities are theoretically dispensable in a way that theoretical entities in science are not; that is that one can always reaxiomatize scientific theories so that there is not reference to or quantification over mathematical entities in the reaxiomatization."[118]

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[117] H. Field, *Science Without Numbers*, p. vii.

He argues that all a good mathematical theory need be is conservative and sufficiently comprehensive. "A mathematical theory M is conservative if and only if for any assertion A about the physical world and any body N of such assertions, A doesn't follow from N+M unless it follows from N alone."[119] Neither comprehensiveness nor conservativeness require truth. A mathematical theory with both comprehensiveness and conservativeness can be useful in science in two ways. The mathematics could make deductions of nominalistic conclusions from nominalistic premises easier, or it could be essential in the formulation of premises for some extra-mathematical theory. Field's project is to show that mathematics is conservative in the way he defines it and that mathematics is not essential in formulating scientific theories. Conservative mathematical theories can be useful in the first way even if they are not true. Any nominalistic conclusions reached with the help of a conservative mathematical theory could have been reached without that help. We know this because conservativeness is defined with this feature right in it. But the second use for mathematics is not as easy for an anti-realist to account for. Field attempts to do just this by recasting gravitational theory in nominalistic language, and suggesting that this translation can be extended to other physical theories. In essence what he tries to establish is that mathematics is merely instrumental in both its uses, and science could go on just as well, if less efficiently, without it.

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[118] H. Field, *Science Without Numbers*, p. viii.

There has of course been a chorus of objections to Field's project. Some claim that his translation is not really successful, others that whatever success it enjoys in the case of gravitation theory will not be extendable to other physical theories, especially quantum mechanics.[120] But a possibly more serious objection arises from the notion of conservativeness itself. Field defines conservativeness in terms of the idea of 'follows from.' "Follows from" must be either deductive or semantic consequence. Stewart Shapiro argues that the ambiguity between semantic and syntactic consequence, along with the differences between first and second order languages prevents Field's project from succeeding. His conclusion is that for mathematical and physical theories, "either the mathematical theory is not conservative in the philosophically relevant way or the mathematics is not applicable to the physical theory in the usual way."[121] There is also a concern about whether Field has a right to metalogical results at all, since these normally invoke the existence of some model(s). Field's anti-realism about mathematical objects surely leaves him without the models necessary to prove various metalogical theorems.

The arguments and responses making up the debate surrounding Field's project are difficult and technical. Happily, I don't think it is necessary to rehearse them and

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[119] H. Field, "Realism and Anti-Realism about Mathematics," p. 7

[120] A nice summary of the objections to Field's position is given in B. Linsky and E. Zalta, "Naturalized Platonism and Platonism Naturalized," p. 529-530. Also see J. Burgess "Why I Am Not A Nominalist," D. Malment "Review of Field *Science without numbers*," A. Urquhart "The Logic of Physical Theory," B. Hale "Nominalism," D. Papineau "Knowledge of Mathematical Objects," and chapter 4 of M. Resnik *Mathematics as a Science of Patterns.*

[121] S. Shapiro, "Conservativeness and Incompleteness," p. 88

adjudicate the present and potential further success of Field's project. This is because Field concedes too much to the indispensability argument. His fictionalism is not consistent with constructive empiricism. He has two objectives: to show that mathematics is conservative and to translate physical theory into a nominalistically acceptable language. Conservativeness is important to Field because if it holds of a mathematical theory relative to physical theory then we know that any consequences we draw from a physical theory with the help of mathematics could have been drawn without the mathematics. But translating physical theories so that they no longer make reference to mathematical entities is just as important. Both parts of his project are necessary because Field concedes that if mathematics is indispensable to science then we have good reason to believe that it is true.[122] He seeks to show that the uses of mathematics as an inference tool and in formulating theories are both eliminable.

However, from the point of view of constructive empiricism, Field's project is not necessary. Constructive empiricism already rejects an analogue to the indispensability argument. We saw in the last chapter that scientific realists argue for belief in unobservable entities in a manner analogous to the indispensability argument; the instrumental success of science is taken to be evidence for not just the empirical adequacy but the truth of its theories. This is just like arguing that the success of science is evidence for the truth of the mathematical theories that are a part of its theories. Constructive empiricism rejects this reasoning. Usefulness does not entail truth, not for unobservables physical entities, not for abstract ones. Hence, a constructive empiricist is not going to find it necessary to show that the use of mathematics in science is dispensable. He need only show that science's use of mathematics does not require its truth. Mark Balaguer mentions a line of argument consistent with my claim here. Of the mathematical anti-realists he says that they "might try to solve their problem by adopting a general instrumentalism, i.e., by claiming that our empirical theories are not true, but merely empirically adequate." Further, he claims, "if fictionalists make this move, they will not have a problem with respect to applicability."[123] Now the reason Balaguer gives for thinking that instrumentalists will not have a problem with applicability is instructive. He goes on to say that

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[122] The first chapter of *Science Without Numbers* is devoted to showing that the usefulness of mathematical entities in science is quite different form the usefulness of (other) theoretical entities. This is clearly because in his estimation the theoretical indispensibility of theoretical entities in scientific theories provides sufficient reason to believe in theoretical entities. He says, "...subatomic particles are theoretically indispensable; and I believe that that is as good an argument for their existence as we need." (p. 8)

> (a) on this line, empirical theories are every bit as fictitious as mathematical theories, and (b) its entirely obvious how one fiction could be applicable to another. (All you have to do is make up the two fictions in the right way; thus, within a general instrumentalism, the fact that mathematical theories are applicable to physical theories is not more surprising than is the fact that *Rambo II* is applicable to *Rambo III*.)[124]

Clause (a) of Balaguer's characterization of instrumentalism is not entirely true of constructive empiricism's view of scientific theories—they are not mere fictions in the way that we might understand mathematical theories to be; depending on what the world is like, they are true or false. However, his claim about the relation between mathematical and physical theory seems on the right track. Theory construction is responsive to not only the phenomena but also pragmatic considerations of simplicity, theoretical unification and so on. Included in the scope of this process will be mathematical choices; the choice (and perhaps development) of mathematical models and techniques is part and parcel of theory construction. Thus what Balaguer says about *Rambo* looks applicable to the relation between the strictly mathematical and the physical portions of theories: It is no surprise that the mathematics is applicable, they were built to ft each other that way.

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[123] M. Balaguer, "A Fictionalist Account of the Indispensable Application of Mathematics," p. 297.

[124] M. Balaguer, "A Fictionalist Account of the Indispensable Application of Mathematics," p. 297

Constructive empiricism asserts that we only have good enough reason to believe what successful scientific theories are empirically adequate, that what they say about the phenomena is true. Since mathematical entities are not among the phenomena, the success of our scientific theories does not give us reason to believe that mathematics is true. So, if we accept constructive empiricism, we do not have to be committed to Field's project. Even if mathematics is more than just useful, even if it is essential in deriving conclusions from physical theory, we are compelled only to take this as evidence of the acceptability of mathematics. It is acceptable because it is useful—this much requires empirical adequacy—but usefulness, even indispensibility, is not good enough evidence for truth. As constructive empiricists we insist that the only legitimate inference from the success of science is to its empirical adequacy. Since mathematical objects are unobservable, empirical adequacy falls short of truth about mathematical objects.

We are, however, still left with the pull of semantics towards realism about mathematical objects. It seems obvious that "1 + 1 = 2" is just plain true. So even if the use of mathematics in science does not require its truth, there is still a strong argument for realism. In this respect Field's project seems a bit beside the point—it does not engage with the semantic argument for platonism. Field claims that it is "clear that there is one and only one serious argument for the existence of mathematical entities, and that is the Quinean argument." To the contrary, it seems clear to me that while the indispensibility argument is of dubious value but we must deal with the semantic argument.

The indispensibility argument is at its most plausible as above, when citing examples of arithmetic statements involving small numbers. This is no accident. And it reveals a continuity between mathematical and scientific theories that should be welcome to constructive empiricism. Our everyday experiences tell us that basic finite arithmetic is true of the world. This observation points to a different strand in the history of the philosophy of mathematics: the empiricism coming from Mill. The idea that mathematical truths are different only in degree of confirmation from other truths of natural science is not one that has had a lot of favor. But as the kernel from which Phillip Kitcher develops his sophisticated mathematical naturalism it has much to recommend it to constructive empiricism.

There are a number of things that make an approach like Kitcher's attractive from the point of view of constructive empiricism. Kitcher's position allows that mathematics might be essential to science. His approach does not, however, involve any variation from standard semantics; his strategy for avoiding belief in abstract objects is to contend that when properly understood, mathematical language isn't really about mathematical objects. He does not suggest an alternative way of interpreting quantifiers or singular terms. Instead he changes the domain over which mathematical variables range. Mathematical statements do not quantify over abstract objects. What they quantify over are concrete operations that we perform in and on the world. According to Kitcher, "arithmetic describes those structural features of the world in virtue of which we are able to segregate and recombine objects: the operations of segregation and recombination bring about the manifestation of underlying dispositional traits."[125]

He rewrites first order arithmetic in the language of Mill arithmetic. This translation generates a set of axioms that quantify over operations rather than abstract objects like numbers or sets. The Peano axioms are embedded in this system with the following:

Primitive Predicates
Ux: x is a one-operation
Sxy: x is a successor operation of y
Axyz: x is an addition on y and z
Mxy: x and y are matchable

Axioms
8: $(x)(y)(z)(w)((Sxy \mathbin{\&} Szw \mathbin{\&} Mxz) \rightarrow Myw)$
9: $(x)(y) \sim (Ux \mathbin{\&} Sxy)$
10: $((x)(Ux \rightarrow Fx) \mathbin{\&} (x)(y)((Fy \mathbin{\&} Sxy) \rightarrow Fx)) \rightarrow (x)Fx$
11: $(x)(y)(z)(w)((Axyz \mathbin{\&} Uz \mathbin{\&} Swy) \rightarrow Mxw)$
12: $(x)(y)(z)(u)(v)(w)((Axyz \mathbin{\&} Szu \mathbin{\&} Svw \mathbin{\&} Awyu) \rightarrow Mxv)$

These axioms alone do not suffice for the development of arithmetic in the usual way. The following additional axioms are necessary:

13: $(\exists x)Ux$
14: $(x)(\exists y)Syx$
15: $(x)(y)(\exists z)Azxy$

All the familiar results in elementary arithmetic can be proved with these axioms; they reinterpret arithmetic so that it quantifies over operations rather than abstract objects like numbers. However, the axioms also generate a serious problem for Kitcher's account. One of Kitcher's aims is to show how mathematics is true even though it is not about abstract otherworldly mathematical objects, hence the move to concrete operations as the real subject matter of mathematics. This move is unproblematic for the first twelve axioms. Since they only require finite domains, they can be satisfied by actual existing concrete operations. However, with the remaining axioms a real difficulty arises. Axioms 13-15 in effect posit infinite domains; any interpretation that satisfies them must have infinite domains. They could not be true otherwise. But this leads us back down the same road to realism.

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[125] P. Kitcher, *The Nature of Mathematical Knowledge*, p. 108. Kitcher's account aims to embrace more than just arithmetic—arithmetic sytematizes elementary concrete operations that we perform on the world, as do other elementary parts of mathematics. More advanced parts of mathematics can be understood as systematizations of operations made possible by mathematical notations.

If we require infinitely many objects to satisfy the axiom system, then these objects cannot be concrete, since there is no guarantee that there are infinitely many of these. There are not the infinitely many concrete operations required to satisfy the theory. So, if the theory is true it is about something other than concrete operations. The obvious solution is to say that the theory is about abstract idealized operations, and this is what Kitcher proposes. But then the account doesn't avoid the realism about abstract mathematical objects it aims to elude; abstract operations seem to be necessary to satisfy the axioms. And if this is right then Kitcher's account is unacceptable to constructive empiricism. It is true that Kitcher is in the position to provide an explanation of the relationship between what mathematics is really about and its application in the world. Mathematics is about abstract operations, idealized from our actual operations in and on the world. Given this the applicability of mathematics is quite natural. It is not different from the applicability of any scientific theory that idealizes from real concrete situations. But while this is true, and it does confer some advantage on Kitcher's theory, the account still really doesn't avoid the problem he aims to avoid - realism about abstract mathematical objects. He has merely substituted one kind of abstract object, numbers or what have you, with another, idealized operations.

Kitcher suggests that the difficulty can be dissolved, and his proposed solution is intriguing. We are to think of mathematics "an idealized science of human operations."[126] The idea seems to be that an idealizing theory somehow won't need to be about objects that exist to come out true. Kitcher says that although

> Mill Arithmetic cannot accurately be applied to the description of the physical operations of segregation, spatial rearrangement, and so forth, that is not fatal to the applicability of Mill Arithmetic. We can conceive the principles of Mill Arithmetic as implicit definitions of an ideal agent.[127]

The applicability of mathematics is explained by the idealizing nature of the theory. It starts from concrete agents operating in and on the world and idealizes them and their operations, generating the Mill Arithmetic (and other mathematical theories) that Kitcher presents. To explain how talk of an ideal agent is helpful with the existence problem, Kitcher draws attention to idealizing theories in science. We are to see mathematics as analogous to these. Like ideal gases, ideal agents do not exist. Moreover, "[s]tatements of arithmetic, like statements of ideal gas theory, turn out to be vacuously true."[128] And thus we get truth without troublesome ontological commitments.

Closer attention to the details of this proposal, however, shows that is not sufficient as it stands. It does not actually make Kitcher's Mill Arithmetic true. There are two ways in which Kitcher might think he can salvage truth for mathematics with his proposal. His talk of vacuous truth suggests one way: by making the whole theory conditional. The other is to take seriously the idea that the ideal agent is a fiction and that the theories are stories. It is this latter view that I favor, but I will return to it below. Kitcher's use of ideal gas theory as an analogue suggests that he means the former. But this won't work.

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[126] P. Kitcher, "Mathematical Naturalism," p. 313

[127] P. Kitcher, *The Nature of Mathematical Knowledge*, p. 117.

[128] P. Kitcher, *The Nature of Mathematical Knowledge*, note p. 117

Consider the idea gas theory that Kitcher himself brings up. He says that the statements of the ideal gas theory are vacuously true. This is right, sort of. The ideal gas law says $pV = nRT$. This law describes a relationship between temperature, pressure and volume in an ideal gas. There are, however, no ideal gases. And so, the law is false. No existing gases satisfy the equation, they all deviate in one way or another. But the very nonexistence of ideal gases allows us to formulate a true ideal gas law, one with a different form. This law is a universally generalized conditional saying that for all x, if x is an ideal gas then $pV = nRT$. The antecedent of the embedded conditional is never true, thus the conditional itself is always true which makes its universally quantified true. And this is the vacuous truth to which Kitcher refers.

The axioms of Mill Arithmetic could be similarly altered with an ideal agent taking the place of an ideal gas. For the axioms to all come out vacuously true they will have to be recast in a different form. So the vacuous truths of mathematics will not be the axioms that Kitcher has given, but universally quantified conditionals. For example, the truth corresponding to 14 is something like:

14a: $(w)(Iw \rightarrow (x)(\exists y)Sxy)$

Where the primitive predicate Iw means 'w is an ideal agent.' As the antecedent of 14a is never satisfied it is vacuously true. This makes Kitcher's account seem very unnatural. We are not going to get the truths we want out of it. It is not the statements of arithmetic, then, that turn out to be vacuously true, but the statements of another conditional theory.[129]

In an earlier essay, Kitcher suggests that possible worlds could be used to establish the validity of ideal operations. There he says, "... there are possible worlds in which arithmetic is true of our physical collectings, and we can legitimately regard our own world as an approximation to such ideal worlds."[130] But this strategy won't work. Possible worlds, if they exist, are abstract objects "Unless they are in a realm of mathematical entities, a circular argument appears unavoidable."[131]

Even worse, this problem shows a serious mismatch between stipulational and referential truth for Kitcher. The theorems of Mill Arithmetic are stipulationally true because of choosing a particular idealization of rudimentary operations. If another idealization were chosen, a different set of theorems might be stipulationally true. Kitcher attempts to make this truth referential by bringing in the notion of idealization—showing the theorems vacuously true. However, it follows from this that (if Mill Arithmetic is consistent), there are universally quantified statements that are stipulationally false, yet by the idealization will be vacuously true, since all universally quantified statement are.[132] For example, in Kitcher's idealization both:

14a: $(w)(Iw \rightarrow (x)(\exists y)Sxy)$

14b: $(w)(Iw \rightarrow \sim (x)(\exists y)Sxy)$

are vacuously true. But 14b is stipulationally false.

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[129] Resnik seems a bit worried about something like this. He also points out that the contraries of idealized generalizations are equally as true. Thus, "both 'all balls rolling down a frictionless plane reach the bottom' and 'no balls rolling down a frictionless plane reach the bottom ' are true." (*Mathematics as a Science of Patterns*, p. 65) The objection is also raised by M. Hand in "Kitcher's Circumlocutionary Structuralism."

[130] P. Kitcher, "Plight of the Platonist," p. 132

[131] A. Drozdek and T. Keagy, "A Case for Realism in Mathematics," p. 331

Maybe the thought Kitcher has is somewhat different. I have been assuming that the goal of Kitcher's talk of an ideal agent is to hold on to the *truth* of mathematics. However, there are indications in "Mathematical Naturalism" that Kitcher might have something quite different in mind. There Kitcher distances himself from the truth of mathematical theories in two ways, both by presenting a non-standard account of mathematical truth and by invoking the notion of storytelling. In a section on the epistemic ends of mathematics, he proposes what looks like a pragmatic theory of mathematical truth. Here he says that mathematical truth "is what rational inquiry will produce, in the long run" and that "there is no independent notion of mathematical truth" in his naturalistic constructivism. True mathematical statements are those that "in the limit of the development of rational mathematical inquiry, our mathematical practice contains."[133] Now this view certainly permits a solution to the current problem. If being true merely means being contained in the mathematical practice at the limit of the development of inquiry, then the objects a mathematical theories quantify over do not actually have to exist in order for those theories to be true. But this will not satisfy anyone who accepts standard semantics, nor anyone who takes seriously the warning that stipulation can not guarantee truth. Defining truth for mathematical statements as inclusion in mathematical practice at the end of inquiry means having, in Benacerraf's sense, a combinatorial account of mathematical truth. It produces an account that does not define mathematical truth in terms of the subject matter of mathematical propositions. More than that, such a pragmatic view of truth cannot satisfy constructive empiricism. It is hard to see how scientific theories would be kept insulated

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[132] T. Norton-Smith makes this objection in "A Note On Philip Kitcher's Analysis of Mathematical Truth."

[133] P. Kitcher, "Mathematical Naturalism," p. 314

from the consequences of such a view of truth. If they can't, scientific statements (like those about ideal gases!) are true simply as accepted parts of scientific practice at the end of inquiry, then the constructive empiricist arguments against scientific realism have been undercut. Whatever the attractions of such a pragmatic view of truth, it cannot be a part of constructive empiricism, and cannot be used by constructive empiricism in constructing an account of mathematics.

But we don't have to entirely reject Kitcher's account. We can refuse to follow his rejection of an independent notion of mathematical truth and still hold onto the basic motivation of his theory. This forces a division, not unlike Hilbert's, between finite and infinite mathematics. It also forces us to supply an alternative explanation of the exact nature of the idealization that comes from reflecting on our operation in and on the world. Happily an alternative strategy is available. There is a fictionalist aspect of Kitcher's position that can be used to undermine the objection that his theory requires abstract objects, a strategy that doesn't invoke any nonstandard view of mathematical truth. This is found in the second way in which Kitcher distances himself from mathematical truth. Expanding on the his idea of mathematics as an idealized science of human mental and physical operations, Kitcher invokes the notion of storytelling:

> One way to articulate the content of the science is to conceive of mathematics as a collection of stories about the performances of an ideal subject to whom we attribute powers in the hope of illuminating the abilities we have to structure our environment.[134]

Elsewhere in the "Mathematical Naturalism," Kitcher reiterates the claim that neither ideal gases nor ideal agents exist, and says that in "both ideal gas theory and in mathematics, we tell stories—stories designed to highlight the salient features of messy reality."[135] Emphasizing the fictional nature of what is going on is Kitcher's attempt to prevent the misunderstanding that attributes to him realism about ideal agents. Ideal agents are, on his corrected reading, merely fictions.

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[134] P. Kitcher, "Mathematical Naturalism," p. 313

Mathematics is just a story, but a useful one. If this is an appropriate way to understand Kitcher's theory, we might ask why operations and ideal agents need to be brought in at all. If mathematics is fiction, if it is about fictions, then why couldn't numbers or whatever standard mathematical object you favor, serve as these fictions rather than the operations of an ideal agent? Mathematics could be a set of stories about numbers or sets or groups or whatever fictions make the most sense in any context. If it is, then Kitcher's formulation of Mill Arithmetic as a theory of operations is not necessary. However, numbers or sets can not do everything that operations can. Operations play a very important role: they explain the usefulness of these stories, mathematical theories. Mathematical theories, although they are merely stories, are useful precisely because they idealize our operations on the world. The world fits into the model that arithmetic provides. Arithmetic is empirically adequate.

The idea that mathematics is fiction is bound to face strong opposition. Where, for instance, does this leave mathematical knowledge? Knowledge requires truth, so saying that mathematics is false would seem to entail that there is no such thing as mathematical knowledge. Kitcher's analogy between mathematics and idealizing physical theories suggests another picture. Some of mathematics turns out to be straightforwardly true of the world - that which makes finitistic claims about operations in the world. But another kind of mathematical knowledge is possible: knowledge of what the idealizing theories say. This is, so to speak, knowledge of the story. As we can know things about the adventures of Sherlock Holmes we can know things about arithmetic - the adventures of the idealized agent, if you like.

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[135] P. Kitcher, "Mathematical Naturalism," p. 324, n. 33

Another objection that could be raised against the analogy with fiction is that mathematics is constrained in a way that fiction is not. Anything goes when you are writing a story, but not when you are doing mathematics. Kitcher has the advantage of a plausible story about the way in which the stipulations involved must be constrained. They are to generate an agent who is an idealization from finite agents in the actual world. Kitcher suggests that the axioms of mathematical theories can be thought of as true because they implicitly define the notion of an ideal agent.

It is also clear that in a way the claim that mathematics is fiction just pushes back the semantic question. Perhaps mathematical theories and fictions can be fruitfully compared, but we are still left with the semantic question. What must be provided is an account of the semantics of stories, of fictions, and one that is acceptable both generally and to constructive empiricism. Fictions provide semantic and ontological puzzles of their own. Simply saying that mathematics is fiction or like fiction simply pushes back the questions. What is the status of an entity in a fiction? How can we give a semantics for fiction that does not require commitment to fictional entities? These questions are the task of the next chapter. They must find answers that are congenial to constructive empiricism if the fictionalist strategy can be used to develop an account of mathematics. We cannot use new abstract objects—fictional objects—to get rid of old ones—mathematical objects—and still maintain constructive empiricism.

After they are answered, my task in the final chapter is to apply to mathematics the theory and semantics of fiction which has been articulated. The resultant account of mathematics should incorporate the strands that I have pointed to in this chapter—Carnap's distinction between internal and external questions, the formalist notion of mathematics as a game, Kitcher's mathematical empiricism—in a way that allows the full use of mathematical language and distinctions. The philosophy of mathematics must not deny the objectivity of mathematics, not restrict its use by science or logic; but equally it must not be realist about either abstract or fictional objects. Such realism would contradict the epistemological limits that constructive empiricism imposes.

Chapter Three

Introduction

The purpose of adopting a fictionalist position regarding mathematics is to avoid belief in mathematical objects. Specifically, my purpose in adopting a fictionalist account is to respect the empiricist scruples that van Fraassen professes in his writings on science. This aim would be undermined by a theory of fiction that was itself realist. While it is all well and good to say that mathematical objects are mere fictions, this claim by itself does not provide an account of the ontology of mathematics. Dismissing mathematical objects as mere fantasies is insufficient; the role mathematics plays in science and our belief its truth will not let us get away that easily. What, then, does it mean to say that numbers, for instance, are just fictions? To answer this question we must look at fictions in their native territory: fictional discourse.

Among the most plausible accounts of fiction there several that embrace fictional objects of one kind or another. Were any of these the best account of fiction, adopting a fictionalist philosophy of mathematics would not be a good anti-realist strategy. But, I argue, Kendall Walton's anti-realist theory of fiction is in fact the best.[136] Walton's theory uses the notion of make-believe to account for fiction and maintains that fictional discourse is not actually about fictional objects. To show that Walton's theory is the best, I first consider the three major realist contenders: Meinongian, modal realist, and abstract realist theories of fiction.[137] I argue that these alternatives all have problems that make them unacceptable. I then look at the success that Walton's theory enjoys in accounting for fiction and argue that the main objections that have been made against it can be answered and do not undermine it. The final section of the chapter then argues that Walton's theory, though he explicitly rejects linguistic theories of fiction, can and should be interpreted in the light of speech act theory. This interpretation will be used in chapter four to generate a constructive empiricist philosophy of mathematics.

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[136] See K. Walton, *Mimesis as Make-Believe*, "Fearing Fictions," and "Pictures and Make-Believe."

[137] The main proponent of a Meinongian theory of fiction are Parsons, *Nonexistent Objects* and Zalta, *Abstract Objects*. Lewis' "Truth in Fiction," lays out the modal-realist theory, elements of which are also adopted by Gabriel in "Fiction: A Semantic Approach." The abstract object view is held by P.

Fiction produces some strange data. But the fundamental puzzle fiction poses asks for a semantic account of ordinary, apparently assertive, apparently true statements like:

(1) Sherlock Holmes lived at 221B Baker St.

It might be question begging to simply claim that (1) is true and therefore there must be a Sherlock Holmes to make it so, whatever the details of his ontological status. However, ordinary statements like (1) just don't seem to be false. They are bet-sensitive.[138] It is appropriate to say that Sherlock Holmes lived at 221B Baker St. in a way that is not to say he lived at 225 Baker St. Anyone betting on the first claim will win their bet, while anyone betting on the second will lose. Bet-sensitivity is not difficult to account for if one allows that there are fictional objects – it is then just a result of truth and truth is taken care of in the standard way with successful reference and so on. The trick is for a theory that

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Van Inwagen, "Creatures of Fiction," "Fiction and Metaphysics," and "Pretense and Paraphrase." Similar abstract object views are espoused by Nicholas Wolterstorff in *Works and Worlds of Art* and "Characters and Their Names," Robert Howell in "Fictional Objects: How They Are and How They Aren't," and Peter Lamarque in "Fiction and Reality," and "Review of Kendall Walton's *Mimesis as Make-Believe*." Gregory Currie also supplements his make-believe account of fiction with an abstract object ontology in order to account for the truth conditions of some propositions involving fictions (See *The Nature of Fiction* and "Fictional Names."

[138] "There exists in English ,and in every civilized tongue, a sizeable set of sentences that exhibit a peculiar and much ignored property. It is that, although they may contain names and variables (or pronouns) that are empty or have no values… these sentences are nonetheless BET-SENSITIVE" J. Woods, *The Logic of Fiction: A Philosophical Sounding of Deviant Logic*, p. 13.

does not countenance fictional objects. Here straightforward truth will not work as an explanation of bet-sensitivity, since there fail to be straightforward truths about such as Sherlock Holmes. But either way, this is one piece of evidence any theory of fiction must account for. The bet-sensitivity of ordinary statements cannot be ignored.

Clearly bet-sensitivity will have to be central also to an anti-realist view of mathematics. Even the most restrictive philosopher of mathematics will allow that there are mathematical statements analogous to (1). Even if they are false, the utterance of statements, like "Every number has a successor", is appropriate in some circumstances, whereas the utterance of certain other mathematical statements, like "1+1=3", never is.

Another ordinary yet peculiar fact that any theory of fiction must address is the apparent incompleteness of fictional characters. Real objects are complete in the sense that for any property *p*, they either have *p* or they lack *p*. Not so characters in fiction. Sherlock Holmes neither has a mole on his back nor does he not have a mole on his back. Conan Doyle's stories just do not mention anything one way or the other about the status of moles on Holmes' back. This makes Holmes a weird kind of object, if he is an object at all. Every account of fiction owes us an explanation of how and why fictions appear to be incomplete.

Related to the incompleteness of fictional characters is the process by which the explicit content of a fiction gives rise to its full content. When a novel is read, for instance, the content of the story is understood by the reader in some way extrapolating beyond the mere sentences appearing before them. This extrapolation is difficult to characterize. It both falls short of and exceeds the relation of basic logical consequence. When reading a story we make numerous inferences. However, we do not make all possible valid inferences. Some strike us as important and relevant, others don't strike us at all. And, indeed, sometimes we revise a sentence that we have along the way inferred. Understanding a story, getting its full content, seems to be partly a matter of logical inference, partly a matter of applying background knowledge and partly a matter of judgments about relevance. Acceptable theories of fiction must have something to say about this process, must be capable of making some sense of it.

Impossible fictions present a further puzzle. Many stories contain inconsistencies; some thrive on them. Circles are squared, people travel backward in time, enormous insects do not collapse under the pressure of their own weight. There are objects in fictions that have contradictory properties. Yet none of this seems to interfere with the integrity of fictions nor with our appreciation of them. The degree of inconsistency a story can sustain without collapsing under the weight of an inferential avalanche is impressive. But how can this be accounted for? If a fiction contains contradiction, why does it not inflate to contain within itself every sentence? And how can there be the impossible objects apparently required to make these fictions true? Handling these questions is essential for any theory of fiction. Though it seems unlikely that impossibility of this kind infects mathematical discourse or objects, a theory of fiction must nonetheless somehow accommodate them. We cannot tailor a theory of fiction merely for the purpose of theorizing about mathematics. It must be more generally applicable than that.

A further fact: As appreciators, we have attitudes towards fictions. It makes sense to say, for instance, that Smith envies Sherlock Holmes or that Jones admires Superman. Fictional names appear in not only ordinary statements like 1), but also statements of intentional attitude. Fictions are, apparently, the objects of intentional attitudes. Evidently, many statements of the following form are true:

(2) Jones admires Superman.

But this seems to require that there be fictional objects. And if this is right, it seems to also reveal that appreciators can be in some relationship with fictions. It looks as if, while fictional worlds are not a part of the actual world - they are radically separated from it, appreciators somehow have access to them. To explain this is difficult for either realist or anti-realist. Realists generally have to grapple with explaining how it is appreciators can have access to entities that are causally inert or from which they are causally cut off. Anti-realists must somehow explain how access is possible, while anti-realists must explain away the appearance that the truth of 2) requires that there be fictional objects.

Meinongian realism

One line of theorizing about fictions finds its inspiration in Meinong's belief that every thought has an object. One advocate of this approach to fiction is Terence Parsons.[139] He has developed a formal theory of objects, an account of nonexistent objects that opposes the philosophical orthodoxy deriving from Russell's attack on Meinong.

Parsons urges that we allow our ontology to embrace one large class of objects that *are*, which includes not only existent but also nonexistent objects. There are more sets of properties than existent objects - some sets of properties are not possessed by anything that exists. These sets, however, also have objects correlated to them, nonexistent objects.

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[139] I am taking Parsons' account to be representative of Meinongian treatments of fictions. Parsons' account is not the only recent well developed Meinongian theory of objects applied to fictional objects. Edward Zalta presents another in *Abstract Objects: An Introduction to Axiomatic Metaphysics*. Zalta's theory makes use of a distinction between objects exemplifying properties and objects encoding properties in order to circumvent violation of the principle of contradiction, rather than utilizing a distinction between nuclear and extranuclear properties for this purpose as Parsons does. However, while the theories developed by each do have important differences, their general approach to fiction is

Nonexistent objects are just those objects that are correlated with sets of properties that are not correlated with existent objects. Included among this plethora of objects, then, will be all the objects of fiction with which we are acquainted.

To avoid Russell's charge that nonexistent objects are "apt to infringe the principle of contradiction,"[140] Parsons places a restriction on just which sets correspond to objects. To accomplish this, Parsons distinguishes between nuclear and extranuclear properties. Nuclear properties are just the ordinary properties that objects have, but the ones that are crucial to their identity and essential nature. Extranuclear properties are those properties an object has that are in some way not ordinary, that are not crucial to the object's identity. It is only sets of nuclear properties that are correlated to objects.

We are left with a theory of objects that includes an infinity of objects, one for every non-empty set of nuclear properties. Its ontology includes a ready stock of nonexistent objects to which we can refer, so the fact that fictional objects typically don't exist does not mean that we cannot refer to them. Hence all the difficulties that arise in fictional contexts because of the (apparent) failure of reference simply dissolve. A proposition like "Sherlock Holmes is a detective " is semantically on par with "Tony Blair is Prime Minister." Parsons gives the following schema for identifying a fictional object ϕ:

> (*) The ϕ of story s = the object x which is such that for any nuclear property p, x has p if, and only if, the ϕ of s is such that in s it has p.[141]

Sherlock Holmes, on this view, is the nonexistent object that has all and only those nuclear properties that Sherlock

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shared and their treatment of fictions sufficiently similar to make my objections against Parsons of equal force against Zalta.

[140] B. Russell, "On Denoting," p. 484

[141] T. Parsons, *Nonexistent Objects*, p. 55

Holmes has in the Conan Doyle stories. Objects in fictions can be non-existent (Sherlock) or existent (London); they can also be native or immigrant. An object is native to a fiction if the fiction "totally 'creates' the object in question," it is immigrant if it "is an already familiar one imported into the story."[142]

The general solution to the problem of fictional discourse proffered by Parson's account is neat and elegant. He has nice explanation of many of the facts any theory of fiction must be able to account for. The bet-sensitivity of ordinary statements is easily accounted for in terms of their truth. If you bet that Sherlock Holmes lived in London, you win because it is true. Sherlock Holmes is an object that has, among others, the nuclear property of living in London. If you bet he lived in Toronto, you lose because he does not have the nuclear property of living in Toronto. The incompleteness is similarly handled: a fictional object is correlated with the set of properties that it has in the story to which it is native and this set need not be complete.[143] The appearance of impossible objects in fiction is also neatly explained by Parsons' account. There happens to be an inexhaustible supply of impossible objects at his disposal. For every set of nuclear properties containing contradictory properties there is a corresponding impossible object. Tales of square circles and such things present no special difficulty for this account. Further, the intentional attitude statements that we are inclined to make, and assent to, are also accounted for. "Smith admires Holmes," has clear truth conditions involving there being an object called Holmes and Smith having the right kind of attitude towards that object. In a way, Meinongian theories of fiction are tailor-made to handle this aspect of fiction. Meinong was developing a theory that respected his belief that every thought has an object, and Parsons is following suit. That his theory of fiction can explain the fact that intentional attitudes appear to take fictions as objects is hardly surprising.

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[142] T. Parsons, *Nonexistent Objects*, p. 51

[143] R. Howell, "Fictional objects: How they are and how they aren't" and J. Woods, *The Logic of Fiction* argue that Meinongian theories have problems accounting properly for the incompleteness of fictional entities. I don't think these arguments are successful, but if I am wrong about this it only strengthens the case against a Meinongian treatment of fiction.

However, all is not perfectly simple and easy. The theory has clear drawbacks as well. First, a general problem: accounting for which truths a fiction generates and how they are generated. The Meinongian finds it difficult to explain how the sentences explicitly in a fiction give rise to a full story. But second, most troubling of all, is the characterization of what authors do that is implicit in the theory. Parsons' account implies that authors are describing objects that are already out there when they create fictions. These problems lead me to conclude that Meinongianism does not provide an acceptable theory of fiction. I will take them up in turn.

First, the problem of generation. When we read or hear a story, we understand a whole lot more than merely the sentences that are read (heard). Every theory of fiction must give some account of this process. But Parsons especially needs to do so. We have seen that identifying fictional objects requires, on Parsons' theory, knowing what properties they have according to the story. These properties will go well beyond just what the actual sentences say and they may fall short in some ways as well.[144] To determine what properties an object native to a fiction has, we must know what the fiction says in total, and not just what the sentences explicitly say. And so, Parsons owes us some account of how to figure out what is true according to a fiction. Without this, we cannot identify fictional objects reliably.

Nonexistent fictional objects have the properties that they are said to have in their native fictions. Hence, figuring out the properties of a native fictional object requires us to establish what its native fiction says. This is not an easy matter in practice, though, of course, the difficulty of the task is not Parsons' alone.

The content of a story should be "what a normal attentive reader understands to be true in the story."[145] This comes from a process of extrapolation when reading and results in a maximal account of the story. What is true in the story is whatever a maximal account explicitly says and nothing else.

After considering a few different principles that might be appropriate for generating a maximal account, and discarding each as either too restrictive or too permissive, Parsons concludes that the major source of interest in principles comes from their failure to do justice to the process of extrapolating to a maximal account.[146] This admission seems right, and it underscores the essential untidiness of understanding fiction.

According to Parsons, building a maximal account turns out to be primarily a matter of amassing a stock of characters. Much of the content of a maximal account will then be truths about these characters that the reading generates. The stock of characters limits the *de re* judgments that can be made in relation to the fiction. Only characters in a story can be said to be such that in the story they are p.

So the stock of characters limits the generation of a maximal account. This cuts things down considerably. Parsons has managed to exclude a huge block of irrelevant sentences from maximal accounts here. However, he has not excluded all

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[144] There is also the further problem of unreliable narrators to contend with. This, however, is an issue I will leave aside.

[145] T. Parsons, *Nonexistent Objects*, p. 175

[146] T. Parsons, *Nonexistent Objects*, p. 179-80

irrelevant material. No limit is set on the inclusion of truths about objects that *are* characters in the fiction. But surely some of these truths are too remote to be considered part of the story. Further, the limitations that characters generate do not give any direction for characterizing the process by which truths not logically entailed by the text are added to the maximal account.

One of the main attractions of a Meinongian theory of fiction is the apparent order and clarity it brings to understanding what fictions are. But this neatness and clarity simply masks what a murky, messy business fiction really is. Once we have established just exactly what a fiction says, the Meinongian theory can then lay out nicely exactly which objects the fiction is about and are its truth makers. *But it bestows no advantage prior to this stage in the process*. And if we are unable to escape the constant reconsideration and revision of what fictional truths a work generates, then the idea that we are talking about some particular fictional object looks less compelling. This objection may not be fatal. The Meinongian could point out that the order and clarity of the theory lies in the homogenous semantics it produces. The messiness of fiction does not have an impact on this, as it is a function of the limitations of our knowledge, not of the nature of fiction itself. But such an argument is weak in this context. Fictions are not independent of people in the way that this argument requires.

The second serious problem for Parsons' theory is the picture it gives of what authors do when they create fictions. The picture we get is strange and unacceptable in a few ways. First, it does not fit with our way of talking about the authors of fictions. Making fiction is a creative act. Writing a story, for instance, typically involves creating characters. To be sure, this does not mean that authors make these characters exist, since the characters do not exist. But authors do, in some sense, create characters. But Parsons' view implies that authors merely identify fictional characters: authors simply describing some nonexistent object(s) already out there. This seems dead wrong.[147]

It may be wrong to describe an author's actions in this way, despite the fact that Parsons account seems to lend itself to such talk. There is a difference between what an author does in creating a fiction, writing a story and what a reader does when they read it. It is perhaps inappropriate to speak as if authors are making assertions when they write fiction, or to speak of the truth conditions of the sentences they inscribe. When Conan Doyle wrote:

(3) Sherlock Holmes lived in London

he was not describing an object, asserting something that was true because there is a certain nonexistent object with the right properties. He was performing some other act. But if this is correct, then something must have changed when someone other than Conan Doyle later says, "Sherlock Holmes lived in London." By hypothesis their utterance is assertive and about the nonexistent object Holmes. So why isn't Conan Doyle's? Nothing has changed, as far as I can determine. At least nothing that is relevant to the claim that only the latter utterance is true *because only it accurately describes a particular nonexistent object*. Parsons can't have it both ways. If what makes (3) true is some nonexistent object out there having the right properties, then it should be equally true when such a statement is made by an author or a reader.

My conclusion from this is that it isn't because there is some object out there that is the denotation of "Sherlock

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[147] If it is right, it raises yet another problem for the Meinongian. As nonexistent objects, fictions are not causally accessible to authors. This means that establishing reference by an act of baptism is impossible. See D. Hunter, "Reference and Meinongian Objects" for a discussion of this. Linsky and Zalta, insist that reference to abstract individuals (including mathematical and presumably fictional objects) is "ultimately based on description alone" (p. 546) and that thus a causal connection to abstract objects is not necessary for knowledge of them. Their theory generates a plenitude of abstract objects—one corresponding to each desription, so reference literally *cannot* fail.

Holmes" that it is right to say "Sherlock Holmes lived in London." The only access we have to fictional objects is through representations. The only evidence we can have for claims we make about them is from those representations. This makes the situation with fictions different from other objects that we have theories about. It is because Conan Doyle himself wrote certain things, and these make it right to say that Holmes lived in London.

In *Nonexistent Objects* Parsons gives no general argument for the is/exists distinction.[148] Parsons' view is that the distinction is vindicated by the successful application of the theory he builds with it. However, there are alternatives to Parsons' theory which say that fictional objects exist. Both modal realist and abstract realist theories posit existing fictional objects and take them to be the truth-makers for fictional propositions.[149] Parsons argues that there isn't much to choose between these and his theory: "One approach is to bloat the realm of existence and the other the realm of nonexistence."[150] But the argument for the exists/is distinction, is the success Parsons' theory enjoys in explaining the data. If there are alternative theories that enjoy similar success, without resting on the controversial distinction, isn't the argument undercut? More generally, there *is* a real difference between adding more entities to an ontology of a kind already accepted, and adding a whole new kind of entity. Calling both 'bloating' ignores this difference. I do not wish to endorse the view that fictional objects are the abstract objects of literary theory, but the view does have this much advantage over Parsons' theory: it does not add an entirely new category of object to our ontology.

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[148] R. Howell, "Review of Nonexistent Objects" expresses this concern. He also worries about how principled the nuclear/extranuclear property distinction is: "...I suspect that the nuclear/extranuclear distinction that he really wants to draw is simply that distinction... that will allow his formal theory to proceed consistently..." (p. 168)

[149] See P. Van Inwagen, "Creatures of Fiction," and D. Lewis, "Truth in Fiction." I discuss the views below.

[150] T. Parsons, *Nonexistent Objects*, p. 205.

There is a cost involved in Meinongian theories: that of expanding our ontology to include nonexistent objects, the attendant ontological excess and epistemological puzzles. Were this approach to really simplify and clarify the issues fiction raises, the price might be worth paying. The clarity gained is, however, insufficient. There are other cheaper theories of fiction that fare as well or better than Meinongianism.

Lewis' modal realism

Meinongian views of fiction understand fictional names to refer, but to refer to nonexistent objects. David Lewis takes another tack. He concurs that fictional names refer, but to objects of a different kind: possible objects. Unlike the nonexistent objects of Parsons' theory, these objects exist; they just aren't actual. But Lewis' modal realism nonetheless provides a theory of fiction with similar advantages and problems to Meinongianism. His alternative is to not take descriptions of fictional characters at face value but to regard them as always implicitly including a prefix of the form "In such-and-such a fiction…," and to understands this locution to contain implicit reference to possible worlds. It is this implicit reference that provides the semantic analysis for fictional propositions:

> *A sentence of the form "In fiction f, ϕ" is non-vacuously true iff whenever w is one of the collective belief worlds of the community of origin of f, then some world where f is told as known fact and ϕ is true differs less from the world w, on balance, than does any world where f is told as known fact and ϕ is not true. It is vacuously true iff there are no possible worlds where f is told as known fact.*[111]

To determine if a proposition is (non-vacuously) true we consider all the possible worlds in which all the collective beliefs of the community of origin of the stories come true. Then we look at those in which the stories are told as known fact. What makes some proposition true in the stories is that for each collective belief world there is (at least) one possible world where the stories are told as known fact at which the proposition is true that is more like it than any at which it is false.

Lewis' analysis gives clear explanations of many of the features of fiction that it needs to. Ordinary statements are bet-sensitive because they are implicitly understood to be prefixed by 'in fiction f,' and hence true or false according to the facts about certain existing possible worlds, as laid out by Lewis' analysis. That fictions are the objects of intentional attitudes is also easily explained: fictions are existing, if non-actual, objects, and are thereby able to be the objects of intentional attitudes just as much as anything actual. Explaining the incompleteness of fictional characters turns out to be a little more involved, but just as satisfactory. Fictional characters are not really incomplete, any more than actual objects are; since fictional names refer to possible objects, fictional characters cannot be incomplete. It is not the case Sherlock Holmes neither has nor lacks the property of having a mole on his back. What is incomplete is the set of truths in the Holmes stories about Sherlock himself. In every possible world where Sherlock Holmes exists he is different in various ways, with respect to properties neither mentioned nor implied. In some he has a mole on his back, in others he does not. So, in some possible worlds "Holmes has a mole on his back is true," while in others "Holmes does not have a mole on his back" is true. Neither is true in the

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[111] D. Lewis, "Truth in Fiction," p. 273.

actual world: There are possible worlds where the stories are told as known fact and Holmes has a mole on his back and there are others at which Holmes doesn't have a mole on his back. And there are none where he does which, on balance, differ less from the actual world than all of those where he doesn't. Neither are there any where he has no mole that differ less than all those where he does. Hence, though the idea that fictional characters are incomplete is wrong, the incompleteness of the set of truths about any fictional character is explained.

However, Lewis' theory faces a number of objections. As a realist theorist, Lewis owes us a clear account of the generation of the content of fictions. Not having one will undermine the main advantage of the account - the clarity constituted by its homogenous semantics. But two other problems present a graver challenge to the modal realist analysis. First, the account cannot adequately deal with impossible fictions. And second, a more general problem shared with Parsons and other realist theories, the theory implies an unacceptable characterization of authorial activities and the institution of fiction.

Unsurprisingly, Lewis does not provide a precise account of generation. He tells us that a sentence ϕ is a part of the content of a fiction if and only if ϕ is true at some world that is more like the collective belief worlds of the fiction's community of origin than is any world at which ϕ is false. What it doesn't tell us is how to establish the exact worlds that are the collective belief worlds of the community of origin, nor does it explicitly tell us how to measure the distances between the various worlds that come into play in deciding whether ϕ belongs or not. It might be that what Lewis has in mind is something like the metric he gives for counterfactuals. However, this won't work as a general strategy, since laws of nature do not hold in all fictions. In these cases we will not know how to measure. Without this information, the generation of the content of a fiction is just as messy and murky a business as ever. This undermines the account at least a little. As in the Meinongian case, we have an theory of fiction that gives clear homogenous semantics, but merely hides the essential messiness of fiction.

However, impossible inconsistent fictions present a more serious challenge to Lewis. There are no possible worlds in which the impossible is true. Hence there can be no possible worlds on which impossibilities are told as known fact and there cannot be fictions in which contradictions are (non-vacuously) true. But we know that there are such fictions. In order to allow the analysis to accommodate such fictions, Lewis suggests that they can be divided into consistent fragments and each of these separately subjected to the analysis.[152] Only by splitting an impossible fiction into consistent fragments is the analysis able to get some traction and generate non-vacuous truths. Since there are no possible worlds at which any impossible fiction is told as known fact, in such fictions all propositions are vacuously true. But each consistent fragment of an inconsistent fiction will generate a set of possible worlds at which it is told as known fact and thence a set of propositions that are (non-vacuously) true in it.

There are then two choices as to what to do with the resultant sets of propositions: intersection or union. But both methods must be rejected. First, intersection. Such a method cannot help but result in the loss of a large number of propositions that explicitly appear in the original work. Each proposition that only appears in one part of the story will be missing from the intersection of all the fragments. Thus the vast majority of each of the fragments will be missing. Surely this is unacceptable. And what about union? Well, the problem with this method is less obvious. The union of consistent fragments makes all of what is explicitly in a fiction true in the fiction. It also allows a certain amount of generation of additional truths—each of the consistent fragments will themselves, before the results are combined, generate truths in addition to those explicitly in the text. After union, however, no more inferences can be allowed, or an inferential avalanche will result. Whatever original inconsistency or impossibility the text had is reintroduced into the set produced by the union of the consistent fragments. So it cannot be closed under implication. "We should not even close under the most obvious and uncontroversial implication: the inference from conjuncts to conjunction."[153] The result is that Lewis' theory cannot adequately account for works in which inconsistency is crucial. The theory will count as true that the water in M. C. Escher's *Waterfall* flows uphill, and also that it falls downhill. That the water flows uphill and it flows downhill will not be true. But surely a big part of the point of Escher's Waterfall is exactly the depiction of something that is impossible: that in it the water is flowing uphill and the water is flowing downhill. Neither method is satisfactory.

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[152] D. Lewis, "Postscripts to 'Truth in Fiction,'" p. 277.

A final serious objection to Lewis is that his theory, like the Meinongian, makes authors describers or discoverers rather than creators. The same argument I made against Parsons applies here: Authors create fictions. The realist theory implies that they do not, that they are merely describing objects that are already 'out there.' And if the realist account of the truth of fictional propositions is not meant to apply to what authors do, then an argument is needed to establish that authors' activities are somehow exempt. And there does not appear to be reason to accept that they are. As in the case of the Meinongian theory, I conclude that we are not left with an acceptable account of fiction.

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[153] D. Lewis, "Postscripts to Truth in Fiction," p. 278.

Fictions and abstracta realism

But even if we reject non-existent and merely possible objects we might still be realist about fictional objects. Peter Van Inwagen has an account of fiction that fits this general ontology.[154] For him, the objects that make statements about fiction true are existent and actual, but abstract. They are analogous to the theoretical entities in scientific theories; fictions are the theoretical entities of literary criticism. Van Inwagen derives a familiar argument from his analogy with scientific theories. Just as in scientific theories, there are truths that cannot be expressed without the special vocabulary of literary theory.

> And, sometimes, if what is said in a piece of literary criticism is to be true, then there must be entities of a certain type, entities that are never the subjects of non-literary discourse, and which make up the extensions of the theoretical general terms of literary criticism.[155]

Van Inwagen calls all of these entities creatures of fiction. Creatures of fiction exist and obey the laws of logic. They are what make (some) statements of literary criticism true.

Van Inwagen's theory fundamentally distinguishes between sentences of literary criticism, some of which are true, and what I have been calling fictional sentences, all of which are false. His proposal appears to generate a problem for describing what authors do. The objects that make certain fictional propositions true are the theoretical, and thereby abstract, objects of literary criticism. But this claim makes it look like authors are making silly mistakes when they inscribe the sentences that they do. If the character Sherlock Holmes is an abstract object, then he can hardly be a detective, or a man for that matter. The proposition "Holmes is a detective" comes out false on Van Inwagen's view. So, if we understood Conan Doyle to have been asserting this proposition when writing the Holmes stories, then Conan Doyle *was* doing something silly and mistaken. Conan Doyle should not be so understood - he was not making any assertions, but doing some other thing(s). Since sentences in fictions are not used by their authors to make assertions, they are not, Van Inwagen maintains, about anything at all. So they are not about creatures of fiction. In particular, when Conan Doyle wrote the Holmes stories he was not writing sentences about Sherlock Holmes. On the other hand, however, the sentences of literary theory are about creatures of fiction - characters, plots, novels and so forth. Assertions are made with these sentences. Thus they are about something, and some of them are true.

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[154] P. Van Inwagen, "Creatures of Fiction," "Fiction and Metaphysics," and "Pretense and Paraphrase." Similar abstract object views are espoused by Nicholas Wolterstorff in *Works and Worlds of Art* and "Characters and Their Names," Robert Howell in "Fictional Objects: How They Are and How They Aren't," and Peter Lamarque in "Fiction and Reality," and "Review of Kendall Walton's *Mimesis as Make-Believe*." Gregory Currie also supplements his make-believe account of fiction with an abstract object ontology in order to account for the truth conditions of some propositions involving fictions (See Currie, *The Nature of Fiction* and "Fictional Names.")

How, in this case, can Van Inwagen account for the bet-sensitivity of ordinary statements? Clearly, his theory makes them false, so it is a matter of explaining the difference between those like "Holmes is a detective," which are false yet bet-worthy, and those like "Holmes is a dog," which are both false and not bet-worthy. In order to do this, Van Inwagen makes two moves. First he introduces a new relation that he calls 'ascription.' This is a three-place relation that holds between a property, a creature of fiction and some work of fiction or part thereof. Ascription is a primitive relation,

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[111] P. Van Inwagen, "Creatures of Fiction," p. 303

according to Van Inwagen.[156] Second, he argues that the sentences in critical contexts, though they look just like ones appearing in fictional contexts, behave quite differently. In critical contexts, these sentences are used to express propositions that can be true, propositions about the relation of ascription holding between triples of properties, creatures of fiction and works of fiction. The proposition that “Holmes is a detective” seems to express is false. Holmes is an abstract object and cannot have the property of being a detective, since that would require, for instance, also having the property of having a location in space. But the proposition “Holmes is a detective” expresses in critical contexts *is* true. That proposition is the one also expressed by “The Holmes stories ascribe being a detective to Sherlock.” So what makes “Holmes is a detective” bet-worthy is that it is used to express a true proposition. However, “Holmes is a dog ” is false, whether it expresses the proposition that Holmes is a dog or the proposition that the relation of ascription holds between Holmes, being a dog and the Holmes stories. So “Holmes is a dog” is not bet-worthy.

Making ascription a primitive relation and leaving it unexplained conceals a serious problem in Van Inwagen's theory, which I address below.[157] But before making my objections, I will look at how the theory is able to explain some more of the data that fiction presents. Van Inwagen easily explains the incompleteness of fictional characters. Creatures of fiction are in fact not incomplete - they either have or fail to have each and every property. The apparent incompleteness of creatures of fiction is explained by the incompleteness of works of fiction. Works do not, for every character in them and every property either ascribe that property to that character or ascribe the lack of that property to that character. For instance, "Holmes has a mole on his back" and "Holmes does not have a mole on his back" both are false, even though Holmes is not an incomplete object. How? In a critical context these sentences are both expressing propositions claiming that ascription holds between a property (either having-a-mole-on-your-back or failing-to-have-a-mole-on-your-back), the creature of fiction we call Holmes, and the Sherlock Holmes stories. But in neither case does the claimed relation in fact hold. Neither property is ascribed to Holmes in the stories, so both propositions are false. The creature of fiction we call Holmes in fact does not have a mole on his back; this creature of fiction, like all of them, is an abstract object, and as such, lacks all properties that require concreteness of their possessors. Failing to have these does not make them incomplete, quite the opposite.

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[156] P. Van Inwagen, “Creatures of Fiction,” p. 306

Impossible fictions do not present a problem either, and for the same sort of reason. There are not any impossible creatures of fiction, but works fictions representing impossibilities do not require that there are any. Creatures of fiction do not exemplify the properties ascribed to them in works of fiction. Just as in the case of incomplete ascription, the ascription of impossible or contradictory properties can occur without the creatures of fiction involved having impossible or contradictory properties. A work ascribes the property of flowing uphill to a stream and also ascribes to it the property of flowing downstream. We are not thereby required to take it that there is a stream flowing upstream and flowing downstream in order to make it true that the stream flows upstream and true that the stream flows downstream. The propositions that are true are the propositions that ascription

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[137] S. Feagin "On Fictional Entities" draws attention to another problem related to ascription that Van Inwagen may have, arguing that it is inconsistent with the Quinean ontology he argues for in "Fiction and Metaphysics."

holds between the respective properties, the stream and the work of fiction.

Generation does not appear to constitute a special difficulty for an account like Van Inwagen's. While the question is one he does not address, the theoretical objects of literary fiction are not individuated by what the content of a work of fiction is. So Van Inwagen has no special burden to give an account of how the explicit content of a work generates its full content.

However, there are some real problems with an account like this. One is what it implies about fictions as objects of intentional attitudes. Many sentences like:

(4) Smith admires Holmes.

express propositions that are true. However, Van Inwagen's account has no clear way of explaining why or how this is. There seem to be two possibilities. Perhaps sentences like this should be treated on the model of sentences in critical contexts that express propositions regarding ascription. But this won't do: there is no claim being made about some work of fiction ascribing a property to Smith, or to Holmes. More plausibly, maybe sentences like the above should be treated as expressing propositions about the relation they explicitly mention, namely, the very propositions they appear on the surface to express. But this won't do either. The name 'Holmes', on Van Inwagen's account, denotes a certain abstract, theoretical object. Surely it isn't *this* object that Smith admires. But if it isn't Holmes (the theoretical object) that Smith admires, what makes a proposition like "Smith admires Holmes" true?

The only way out of this difficulty that I can see for Van Inwagen is to insist that the sentence "Smith admires Holmes" is to be treated as parallel to sentences appearing in fictional works, as not being about Holmes, as not being about anything. Think of it as having originated from an original utterance by Smith: "I admire Holmes." And think of Smith's action in this utterance as the same kind of thing as Conan Doyle's when he wrote the Holmes stories. Smith is not asserting anything in this utterance, at most he is simply pretending to assert something, writing a very short story featuring himself and Holmes as characters. This move allows sentences like "Smith admires Holmes" to be treated on the model of critical discourse, as expressing propositions about the ascription relation holding between a creature of fiction, a property and a work. But this doesn't seem plausible. First of all the original sentence really does seem to be about a relation holding between Smith and Holmes, not about ascription. Moreover, often an explanation of the admiration is supplied: "Smith admires Holmes because he is such a good detective." It is even less plausible to consider both "Holmes" and "he" in this sentence to refer to an abstract object. And also even less plausible to construe this case as akin to writing a fiction.[159]

Although this is not the case on the model Van Inwagen suggests for critical discourse, an utterance of "Smith admires Holmes" does seem to have more in common with an utterance of something like "Holmes is Conan Doyle's most fully developed character" than with any of the sentences appearing in the Holmes stories. They share an externality to fiction, and seem to be fairly straightforward assertions. Ignoring this similarity and treating statements of intentional attitude that take creatures of fiction as objects as parallel to fiction making moves Van Inwagen's theory towards a pretense account of fictional discourse. If this is where his theory has to go to account for these statements, it is not clear why we should stop short at critical discourse. Why should we not instead treat all discourse with apparent reference to creatures of fiction as on par with fiction making? Van Inwagen wants to hold onto the truth of (some of) the propositions of literary theory, but any move that denies the truth of all propositions like "Smith admires Holmes" has to weaken the case he has for this.

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[159] This is pointed out by F. Kroon in "Make-Believe and Fictional Reference," p.212.

Finally, and perhaps fatally, Van Inwagen's theory fails to account for the relation that holds between the data that literary theory is intended to explain and the theoretical entities it posits. The failure is concealed by his refusal to explain in any real way the relation of ascription. This problem is a variant of an objection I have raised against both Parsons and Lewis—they make no sense of what the creators of fiction do and its connection to the interpretive enterprise. Van Inwagen grounds his account in an analogy between literary theory and science. Just as our belief in the truth of (some) scientific theories requires us to believe in the theoretical objects they posit, so our belief in the truth of (some) literary theories requires us to believe in the theoretical objects of those theories. This analogy needs to be taken further, however. In the case of scientific theories, theories explain observations by going beyond them in various ways and positing theoretical entities. Theoretical entities in scientific theories are understood to have some specified relationship(s) with the observations and data they help explain. It is because they are taken to have these relationships that they play the explanatory roles they do, and we think we have reason to believe in them. But is this also so in the case of literary theory? To establish that it can't be, all we need to do is think about the various elements involved and how the analogy requires them to be related. Van Inwagen has deliberately, in order to avoid other difficulties, cut any relation that could plausibly be said to hold between the theoretical object—creatures of fiction—and the data they are meant to help explain: materials we take to be fictional and the actions of those who produce them. The sentences of fictions are not about anything. Not about anything. Then how can creatures of fiction be used to explain the materials we identify as fictional, let alone the actions of creators? Creatures of fiction play no role beyond merely making propositions of literary theory true.

In a way, Van Inwagen's theory seems to combine the disadvantages of both realist and anti-realist accounts. By making all ordinary statements and intentional attitude statements false, his theory is forced to provide explanation of their apparent truth, one of the disadvantages of anti-realist accounts and a task most realist theories are able to avoid. But he also has to contend with an awkward picture his theory paints of the institution of fiction and, in particular, of what authors are doing. This is a major problem for all realist theories. Van Inwagen's theory strains the analogy he draws to science to the breaking point. All in all, the abstract realist position does not give an adequate account of fiction.

Indeed, none of the realist theories considered give an adequate account of fiction, so I will now turn to an anti-realist account of fiction that promises to be more successful and also to supply the conceptual tools to account for mathematics within the context of constructive empiricism. Even if they were successful as theories fiction, they would not be acceptable to constructive empiricism since they involve agreeing that we can make, and know we are making, true claims about entities that are beyond any possible experience. It is a good thing then that none provides a perfect account of fiction.

Fiction and Make-Believe

There are a group of theories of fiction that share a basic approach to fiction emphasizing the role of make-believe in the institution of fiction. Kendall Walton's account of fiction is one such theory.[159] Though no currently developed theory of fiction is without difficulties, Walton's does provide an account of both the kinds of things we say about fictions and the activities of both producers and consumers of fictions. According to Walton fictional names to fail to refer. This makes accounting for many of the features of fiction more complicated and difficult than it would be otherwise. But it results, I'll argue, in a theory that is more faithful to fiction than do the others that have been considered.

Kendall Walton's *Mimesis As Make-Believe* is an exploration of the workings of the representational arts. Walton begins his theory of fiction with the observation that fictional works have a role in make-believe. Appreciators use paintings and novels as props in games of make-believe. It is to make-believe, he contends, that we must trace the roots of works of fiction. The account is broader than many in the range of materials that it treats as fictional.[160] Walton begins with the explicit aim of bringing works of all kinds of representational arts under the umbrella of fiction. Other theorists of fiction start with literary fiction and at most extend their accounts to other kinds of works, frequently not doing even this. But any account that is unable to deal with non-linguistic fictional materials is to that extent inadequate. The capacity to deal seamlessly with non-linguistic fiction confers an advantage on Walton's theory.

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[159] Others are presented by G. Currie in *The Nature of Fiction* and G. Evans in *The Varieties of Reference*. In contrast to Walton's, Currie's theory holds that something is fiction because it is the product of a distinctive speech act, the fictive. Fictive speech acts are explained in terms of make-believe.

[160] His theory is also broader than one originating with simply a concern for semantic and metaphysical issues. Although aesthetic questions are outside the range of my interest here, and thus will not be taken up, it is worth noting that the integration of these concerns in Walton's theory makes it more plausible than one that can not address questions in aesthetics and the philosophy of art. As he says an account the institution of fiction's "logical, semantic, and ontological structure that leaves mysterious why there should be an institution with that structure ought to be highly suspect." (K. Walton, *Mimesis as Make-Believe*, p. 6)

The cornerstone of Walton's account of fiction are games of make-believe. These "are one species of imaginative activity; specifically, they are exercises of the imagination involving props."[161] When real things are props in games of make believe they generate fictional truths; it is in virtue of real things acting as props in games of make-believe that there are fictional truths. Works of fiction are props used in games of make-believe used in just this way.

Walton's use of the notion of a fictional truth needs some elucidation, since the theory doesn't countenance fictional objects. Fictional truths certainly aren't truths about fictional objects. Strictly speaking, many of them are not truths at all. A fictional truth is a proposition that is fictional, independent of its truth or falsity. Certain things are pretended in games of make-believe, they are imagined. In general, a proposition is fictional in a game of make-believe if there is a prescription to imagine it. If one of the prescriptions that constitute a game of cops and robbers is to imagine that Billy is a cop, that he is a cop is fictional in the game. Similarly, the prescriptions to imagine that constitute the games of make-believe associated with a story or a painting also generate fictional truths. It is fictional in the *Lord of the Rings*, for example, that there are hobbits.

Right away it is clear how this can allow Walton to account for the bet-sensitivity of ordinary statements. For Walton, ordinary statements about fiction are linked to what he calls authorized games of make-believe. Any work has authorized games associated with it and these games warrant numerous fictional truths. Ordinary statements are those that games authorized for a particular work prescribe to be imagined. This gives us the distinction we need. It is false that Holmes lived on Baker St., but we are supposed to imagine that he does in games authorized for the stories he is featured in. On the other hand that Holmes lived in Toronto is not something we are prescribed to imagine. On the basis of this difference, we can account for the bet-sensitivity of ordinary statements.

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[161] K. Walton, *Mimesis as Make-Believe*, p. 12.

Unfortunately, things are not quite so simple. While Walton does not give a detailed account of propositions, he does think those expressed with sentences that contain names have the referents of those names as constituents.[162] This introduces a complication. If there are no fictional objects, there are not any propositions with fictional constituents. So we cannot say that a proposition like "Holmes lived at 221B Baker St." is fictional, since there is no such proposition. We must make sense of bet-sensitivity by distinguishing another way in which such statements can be appropriate or inappropriate. "Ordinary statements are ones that are understood to be such that they might naturally be uttered in pretense in the course of *authorized* games of make-believe."[163] To assert that Holmes lived at 221B Baker St. might be a mistake, but to pretend to refer to someone by means of the name Holmes and to say of him that he lived on Baker St. in the course of a game of make-believe authorized for the Holmes stories, is not. Conversely, it isn't appropriate in the course of such a game to pretend to refer to someone by means of the name Holmes and to say of him that he lived in Toronto. The (in)appropriateness of certain pretendings in the course of games of make-believe is what grounds the bet-sensitivity of ordinary statements.

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[163] K. Walton, *Mimesis as Make-Believe*, p.36.

In uttering "Holmes lived on Baker St." in the course of a game of make-believe one is normally only *pretending* to assert that Holmes lives on Baker St. But one could also actually be asserting something else with the utterance. The utterance could be intended as an assertion about the games of make-believe authorized for the Holmes stories with this utterance. What Walton says is this:

> In general, when a participant in a game of make-believe authorized by a given representation fictionally asserts something by uttering an ordinary statement and in doing so makes a genuine assertion, what she genuinely asserts is true if and only if it is fictional in the game that she speaks truly.[164]

Such assertions are paraphrased thus:

> (5) To utter "Sherlock Holmes lived on Baker St.," is to assert "The Sherlock Holmes stories are such that one who engages in pretense of kind K in a game authorized for it makes it fictional of himself that he speaks truly" (where pretense of kind K is exemplified by the utterance of "Sherlock Holmes lived on Baker St.")

Now (5) *is* a true assertion, and is this is the explanation underlying the bet-sensitivity of ordinary statements. Many truths can be asserted with the utterance of ordinary statements, but there is nothing that guarantees the truth of assertions made in this way. So we have here a way of distinguishing between utterance that will win bets and those that won't. Take for instance the assertion made by an utterance of "Holmes lived in Toronto." This assertion is false. It isn't true that the Holmes stories are such that one who engages in pretense exemplified by the utterance of "Holmes lived in Toronto" in a game authorized for it makes it fictional of themselves that they speak truly. If you bet that

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[163] K. Walton, *Mimesis as Make-Believe*, p. 398.
[164] K. Walton, *Mimesis as Make-Believe*, p. 399.

it is true, you will lose. A similar bet about the utterance "Holmes lived in London" would, on the other hand win.

This feature of Walton's account is central to the philosophy of mathematics I develop in the next chapter. It will ground the kind of judgements we need to make about mathematical propositions without requiring that abstract mathematical objects exist. Just as in works of fiction, mathematical theories have games of make-believe associated with them. And just as in works of fiction, these games make certain utterances appropriate and rule out others, making certain utterances the vehicles for true assertions about mathematical games and others the vehicles for falsehood, without requiring that the objects apparently referred to exist.

The treatment of the apparent incompleteness of fictional characters is parallel to that of bet-sensitivity. Fictional characters cannot be incomplete, since they don't exist. What makes them seem incomplete is the fact that in authorized games for works of fiction the utterance of certain statements is inappropriate (and the assertions they are normally used to make are false), and the utterance of their negations are also inappropriate (and the assertions the negations are normally used to make are also false). For example, in games authorized for the Sherlock Holmes stories it is neither appropriate to utter "Holmes has a mole on his back," nor is it appropriate to utter "Holmes does not have a mole on his back." The Holmes stories are not such that one who engages in pretense exemplified by the utterance of "Holmes has (does not have) a mole on his back" makes it fictional of themselves that they speak truly. The work does not authorize either of these assertions. For this reason it appears that fictional objects are incomplete, but what is really incomplete is the set of utterances in a game that can be used to assert truths—there are many contradictory pairs neither of which can be used to assert a truth. Unlike incomplete characters, this kind of incompleteness is not problematic at all. There is nothing unusual about a game that does not dictate a complete set of utterances.

But how is it that the set of fictional truths associated with a work get generated? Walton owes us an account of how fictional works, as props, generate fictional truths. Not every fictional truth associated with a work can be read off its surface. In the case of linguistic fictions, the content of a story goes well beyond the content of the sentences inscribed and may even fall short of them, yet we are somehow able to get the content of the story, more or less, from the inscribed sentences.

One possibility is to just include all those sentences that follow logically from those explicitly in the story. Understanding a fiction, getting its full content, seems to be partly a matter of logical inference, but also a matter of applying background knowledge and making judgments about relevance. Walton suggests two principles that operate to generate fictional truths: the Reality Principle and the Mutual Belief Principle.[165] These principles go some way towards explaining the process of generation, but cannot be the whole story. Walton himself admits that they "do not even come close, either separately or together, to providing a systematic, comprehensive account of the mechanics of implication."[166] They both under- and over-generate fictional truths.

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[165] K. Walton, *Mimesis as Make-Believe*, pp. 144-169. The principles are also discussed in Woods, *Logic of Fiction*, Lewis, "Truth in Fiction," and Wolterstorff, *Works and Worlds of Art*.

[166] The Reality Principle is formulated thus:

> If $p_1,\ldots, p_n$ are propositions whose fictionality a representation generates directly, another proposition, q, is fictional in it if, and only if, were it the case that $p_1,\ldots, p_m$ it would be the case that q.

While the Mutual Belief Principle tells us that:

> If $p_1,\ldots p_n$ are the propositions whose fictionality a representation generates directly, another proposition, q, is fictional if and only if it is mutually believed in the artist's society that were it the case that $p_1,\ldots, p_n$ it would be the case that q.

So Walton's theory does not provide us with neat and encompassing set of principles by which fictional truths get generated. This needn't, however, be a serious drawback, either in general or for the case of mathematics, as I will discuss in the next chapter. For mathematics, the generation problem is far less serious, as, unlike regular stories, mathematical theories almost certainly are closed under logical consequence. And it is equally unlikely that they require going beyond logical consequence. In general this isn't true of fictions, but Walton not only openly admits the untidiness of generation and the consequent uncertainty of interpretation, his account has a built in explanation of it. Fictionality consists in a prescription to imagine. Thus the fictionality of a proposition is dependent on the rules of the game of make-believe in which the work the proposition appears in is a prop. Looked at this way, deciding whether a proposition is fictional or not is a matter of pinning down which is the correct game for a particular work. And doing this, surely, is uncertain and untidy work.

We can see how the usual factors come into play here. In principle, a work can be used as a prop in an infinity of different games of make-believe. Some games are, however, more appropriate than others: they are more like what we believe the creator of the work either had in mind or could have had in mind, they are more natural relative to the obvious features of the work, they fit better with the available precedents, they belong to a family of games that have been played with similar works, and so on. Moreover, works of fiction are not about fictional objects that are in some way 'out there.' This explains both flexibility of interpretation and its ongoing nature - interpretation is not a matter of once and for all getting the facts straight about the objects of the fiction.

(K. Walton, *Mimesis as Make-Believe*, p. 164.)

But neither is it a matter of anything goes. The kinds of considerations I mentioned, and others, are brought to bear in deciding how good an interpretation is.

Walton's theory has little trouble with impossible fictional objects. Since the existence of fictional objects is in general denied by Walton, so is the existence of impossible fictional objects. His theory does not have to explain how there can be impossible objects. However, there still needs to be an explanation of inconsistent fictions. In M. C. Escher's *Waterfall*, the water flows uphill, and it also, at the same time, flows downhill. The make-believe theory does not require that the waterfall exist, only that it is fictional that it exists. What looks like inconsistency is a result of the rules of the normal authorized game in which *Waterfall* is a prop. In this game it is appropriate to pretend to refer to a waterfall and to say of it that it is flowing uphill. It is also appropriate to pretend to refer to the waterfall and to say that it is flowing downhill. We are supposed to pretend both of these things, it is a central part of the game. So it is fictional that there is an impossible object, and an inconsistency is fictional also—that the waterfall flows uphill and it does not flow uphill. Fictionality is not like truth: it does not disbar inconsistency. So we do not have the serious problem a realist account can face.

Pretense also plays a crucial role explaining intentional attitude statements like:

(2) Jones admires Superman.

Since there are no fictional objects, they cannot be the objects of our attitudes. Jones cannot really admire Superman if there is no Superman. But games of make-believe again provide an explanation of what is going on in such cases. We do not actually have the attitudes that intentional attitude statements report. Like ordinary statements, these are strictly false. However, it is "fictional, when we appreciate novels, plays, films, and paintings that we feel compassion, exasperation, indignation, and so on."[167] So, also like ordinary statements, intentional attitude statement can be uttered to assert true propositions. In this case, what is asserted will be true if it is fictional in the game that the utterer speaks truly. The feelings that Jones experiences during the course of the game can make it fictional that he admires Superman. To the extent that Jones finds himself experiencing feelings of admiration, make-believedly towards Superman, since it is in the context of the game, the utterer of (2) asserts a truth. Once again, then, the notions of make-believe and pretense get used to explain why certain statements appear to be true, and also to generate the paraphrases that are in fact true.

This answer has been perhaps the most common focus of criticisms of Walton's theory.[168] Walton appears to be claiming in this explanation of intentional attitudes that the emotions involved are not genuine. Critics urge that the intensity and quality of our emotional responses to (some) fictions undermine any characterization of those responses as pretended or make-believe. However, Walton can give an adequate response to this objection. He does not in fact deny that appreciation of works of fiction is a genuine emotional experience, not does his theory require that him to. For example, in a case involving an appreciators response to a horror movie, Walton says, "A (normal) appreciators response to the Green Slime movie... does not consist in her genuinely fearing the slime... But her emotional response is real and it is really emotional. It consists primarily in the imaginative experience of participating in a game in which, fictionally, the slime threatens her and terrifies her, an experience which is itself genuinely emotional, and is likely to involve other emotions as well, possibly including fear."[169]

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[167] K. Walton, *Mimesis as Make-Believe*, p. 250.

[168] Criticisms of and concerns about Walton's use of the notion of quasi-emotions to explain intentional attitudes are found in Goldman, "Representation and Make-Believe," N. Carrol, "On Kendall Walton's *Mimesis as Make-Believe*," P. Lamarque, "Kendall L Walton's *Mimesis as Make-Believe: On the Foundations of the Representational Arts*," J. Levinson, "Making Believe," R. Howell, "Review Essay. Kendall L. Walton, *Mimesis as Make-Believe*,"

Peter Van Inwagen raises another problem. He complains that the paraphrases generated by the account do not have the right logical form.[170] The difficulty is that they do not entail the same consequences as what they paraphrase. Van Inwagen presents examples of the following form:

> (10) There is a fictional character who, for every novel, either appears in that novel or is a model for a character who does.
>
> (11) If no character appears in every novel, then some character is modeled on another character.

(10) entails (11) by virtue of logical form. The paraphrases that Walton's theory suggests will both have forms like "To engage in pretense of kind K is fictionally to speak truly in a game of such and such a sort." Hence the paraphrase of (10) will not entail the paraphrase of (11). Van Inwagen objects that this makes them bad paraphrases.[171]

The response to this objection is actually quite simple. Van Inwagen is mistaken about what the aim of paraphrase is in this context. We don't really have a standard case of paraphrase here. Maintaining the logical relationships that hold between (10) and (11) is not necessary. As Walton points out, what the paraphrases are meant to capture is what the speakers assert in uttering (10) and (11), not what, if anything, propositions (10) and (11) themselves express. What makes it seem more plausible that what (10) asserts entails what (11) asserts is that the following:

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[169] K. Walton, "Reply to Reviewers," p. 413

[170] P. Van Inwagen, "Pretense and Paraphrase." See also Goldman, "Representation and Make-Believe."

[171] P. Van Inwagen, "Pretense and Paraphrase."

> (12) Statement (10) entails (11) by virtue of logical form alone.

Although false, (12) can be used to assert a true proposition of the standard form. It is appropriate in the context of a game of make-believe in which (10) and (11) are fictional to utter (12).[172]

Some may not be entirely happy with Walton's theory. It does not provide a set of neat paraphrases that translate away any commitment to fictional objects. Others may find the explanations that it provides for the mere appearance of truth in statements about fictions to be implausible. But it is clear to me that the difficulties encountered by Walton's make-believe account are no more severe than those encountered by the realist theories. Each of these has problems of its own, and all are plagued by an unacceptable picture of the institution of fiction. It seems to me that, on balance, Walton's theory both generates fewer difficulties and provides a more fruitful approach to fiction than do its competitors.

I now turn to the relationship between Walton's make-believe account of fiction and speech act theory. I will argue that Walton's own theory can be recast in the language of speech act theory, without losing any of its substance. The main purpose this serves is emphasizing that Walton's theory is a pragmatic account of fiction. This does not need much showing. Walton clearly does not distinguish fictional and non-fictional discourse semantically or syntactically. It is our relationship to discourse and what we do with it that distinguishes fiction. That Walton's theory can find natural expression in the language of speech act theory only bears this out. But for me it does more—it provides a language and set of concepts with which to more easily say some of the things I want to say about fiction. In particular it allows me to say more about clearly and more easily some things that I will say in the next chapter about mathematics as fiction, mathematics as make-believe. I should emphasize that I think Walton's theory of fiction is essentially correct; I do not aim to undermine it.

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[172] See *Mimesis*, pp. 416-419

Identifying make-believe as the key to fiction does not *per se* preclude using linguistic strategies to distinguish it from non-fiction. Walton does, however, rule out this kind of approach. His worry is that it is likely to lead to an account that will leave out non-linguistic works. Walton concedes, however, that fiction and non-fiction differ more pragmatically than semantically. This being the case, perhaps the illocutionary force of fictional discourse is a place to start demarcating fiction's boundaries. Where content and truth-value fail to delimit fiction, the notion of an illocutionary act may succeed.

Fiction and Speech act Theory

Speech act theory tells us that language ought to be understood centrally as a function of actions that speakers perform. The properties of sentences and words are to be understood in terms of the roles they play in speakers' actions. The notion of an illocutionary act is fundamental. According to Searle, an illocutionary act is "...the production of the sentence token under certain conditions... and the illocutionary act is the minimal unit of linguistic communication."[173] The crucial difference between just uttering sounds and performing an illocutionary act is that in the latter the sounds produced are characteristically said to mean something.[174] But this claim must be supplemented with an account of what it is for sounds to mean something. Grice argues that to claim someone meant something by an utterance is to say that they intended the utterance to produce some effect in an audience by means of the recognition of this intention.[175] However, by focusing exclusively on the speaker's intentions, Grice leaves out a crucial ingredient in the recipe for meaning. Attention must be paid to the fact that sentences do not get their meanings solely from the intentions of those who utter them but that conventions within dialects about the meaning of terms also play a role. Searle remedies this oversight:

> In the performance of an illocutionary act in the literal utterance of a sentence, the speaker intends to produce a certain effect by means of getting the hearer to recognize his intention to produce that effect; and furthermore, if he is using words literally, he intends this recognition to be achieved in virtue of the fact that the rules for using the expressions he utters associate the expression with the production of that effect.[176]

A hearer's understanding of the speaker's utterance consists in those intentions being achieved. This account diverges from Grice in including the rules associating an expression with the production of the intended effect as the intended vehicle for achieving recognition in the hearer. Any instance of meaning will include both intentional and conventional elements that go into making it up.

Although speech act theory has this, and more, to say about meaning, what it says is just a necessary preliminary to articulating the structure of acts of communication: illocutionary acts. Communication is, after all, treated by speech act theory as a problem in the theory of action. And so, with his account of meaning in hand, Searle proceeds to give an analysis of illocutionary action. He formulates a general set of conditions for illocutionary action. When a particular illocutionary act is performed there are certain prepatory, sincerity and essential conditions that hold. The classification of speech acts laid out by Searle includes assertives, directives, commissives, expressives and declarations.[177] The two kinds of illocutionary act that are of special interest in the context of fiction are assertives and directives. Assertives are those illocutionary acts that have as their point the commitment of the speaker to the truth of the expressed proposition, directives aim at getting the hearer to do something.

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[173] J. Searle, "What is a Speech Act?" p.39

[174] This restriction is for ease of expression only. Verbal utterances are not, as far as I can see, a simpler or easier case than any other kind of utterance. What I say can be generalized to any kind of utterance.

[175] P. Grice, "Meaning."

[176] J. Searle, *Speech acts: An Essay in the Philosophy of Language*, p. 45

Given this understanding of assertion, we might attempt a first theory of fiction: Fiction is simply the failure of assertion. Often enough, fiction is descriptive, it looks assertive yet somehow is not. However, inscribing sentences without asserting them is neither necessary nor, perhaps, is it sufficient for making fiction. Walton argues that we could have "a genre of historical novel in which the authors are allowed no liberties with the facts and in which they are understood to be asserting as fact whatever they write."[178] If so, failure of assertion is not a necessary condition for fictionality. Moreover, there are ways in which assertion can fail that seem not to generate fiction. In lying assertion fails, but lies do not always engender fictions. What I have in mind here is that the aim of the liar is not right for fiction to be created. Lies aim at belief not imagination. If a lie is successful, its audience will believe the propositions the liar appears to assert not merely imagine them. A good lie successfully disguises itself as assertion, whereas a good fiction gives itself away. Perhaps this case of failed assertion is not entirely convincing as non-fiction. If not, envision a circumstance where someone intending to utter, "*There are* free drinks after the talk" instead mistakenly utters "*Are there* free drinks after the talk." This failure of assertion surely does not produce fiction. So, being neither necessary nor sufficient, failure of assertion fails as a criterion for fiction.

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[177] See J. Searle, "A Taxonomy of Illocutionary Acts."

I suggest an alternative: In creating, telling or presenting a fiction, an author is performing directive illocutionary acts with a particular aim: getting the audience to imagine or make-believe what he is uttering.[179] This proposal provides, I think, a persuasive reading of Walton's own make-believe theory of fiction. Given that he argues strenuously against any speech act theory account of fiction, this is a suggestion that Walton would strongly resist. Nevertheless, I will argue that his theory can, without serious alteration, be profitably expressed in the framework of speech act theory.

Fiction is a vehicle for illocutionary action, I am claiming. In creating or telling a fiction, an author is performing an illocutionary act; the nature and aim of this illocutionary act characterizes fiction. Not only that, but Walton's theory of fiction makes works of fiction vehicles for directive illocutionary action. How does this work? Let's review Walton. According to his account, both works and

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[178] *Mimesis as Make-Believe* p. 79

[179] Gregory Currie makes a similar, but importantly different suggestion in *The Nature of Fiction*. Currie argues that what distinguishes fiction is a distinctive illocutionary act: the fictive. However, this claim cannot be maintained. As Searle has argued, "if the sentences in a work of fiction were used to perform some completely different speech acts from those determined by their literal meaning, they would have to have some other meaning." ("The Logical Status of Fictional Discourse," p. 64) But we know that this is not the case. My suggestion, on the other hand, agrees that speech act theory can

propositions can be fictional. To "call a proposition fictional amounts to saying that it is 'true in some fictional world or other.'"[100] However, this articulation of the fictionality of propositions is really just shorthand. Walton's account is anti-realist about fictions, so it is not as if fictional worlds containing fictional characters and objects really exist and make fictional propositions true. Rather, a proposition's fictionality consists in a prescription to imagine it. Such prescriptions are embodied in games of make-believe and the props used in them - these are the generators of fictional propositions.

Works are props; they are fictional if they are used as props in games of make-believe. *Tom Sawyer* is a work of fiction because it gets used as a prop in games of make believe, typically games in which it is pretended that the book tells a true story about a boy named Tom. This activity relates fictional works and propositions: By virtue of their use in games of make-believe, works generate fictional propositions. Any game of make-believe is constituted by a set of rules, implicit, explicit, or both. These specify conditions under which certain things are to be imagined. For instance, the typical game of make-believe played by readers of *Tom Sawyer* involves rules that specify, among other things, that the sentences on the pages are to be read as describing events and that the reader is to imagine that these events really happened and are (mainly) truthfully described. Games of make-believe involving works of fiction have rules that, in concert with the work, generate propositions that are to be imagined. In the case of *Tom Sawyer*, we have a book that, together with the rules of the game played with it, generates to-be-imagined propositions like "There was a boy named Tom"

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illuminate the nature of fiction, without making Currie's mistake of taking the 'fictive' to be a unique speech act.

[100] K. Walton, *Mimesis as Make-Beleive*, p. 35

and so on. Thus far, this is pure Walton. But the last point has a crucial implication: The prescriptions to imagine, which constitute the fictionality of propositions, entail that works of fiction are vehicles for directive illocutionary action. Prescriptions are a kind of directive. Thus works of fiction are utterances used to perform directive speech acts.

Consider a simple example. Assume the following proposition is fictional:

> (13) Two great yellow caravans had halted one morning before the door and men had come tramping into the house to dismantle it.

What it means for this proposition to be fictional, according to Walton, is that there is a prescription for us to imagine it. When we recognize that it is fictional, we recognize that we are to imagine it. But what does this amount to? There are marks on the page that we recognize as having a certain meaning. That is, we recognize these marks as falling under certain rules that specify conditions of their utterance and what they count as. In other words, we recognize these marks as a conventional means of achieving the intention to produce the effect of recognizing that the state of affairs "Two great yellow caravans had halted one morning before the door and men had come tramping into the house to dismantle it" holds.

If the proposition were merely being asserted, this is about all there would be to the story. However, this is supposed to be a fictional proposition. So, what we must further recognize is that we are intended to imagine the state of affairs, not believe that it actually holds. The directive here is:

> (14) Make-believe that two great yellow caravans had halted one morning before the door and men had come tramping into the house to dismantle it!

As a speech act, (14) comes off successfully if and only if the felicity conditions for the directive are satisfied. Namely:

Prepatory Condition:	The hearer is able to (14), and it is not clear to both the speaker and hearer that the hearer will in the normal course of events (14)
Sincerity Condition:	The speaker wants the hearer to (14)
Essential Condition:	The speaker's utterance counts as a request to (14)

A problem is immediately apparent: The essential condition is not satisfied. The utterance, (13), does not count as a request to (14). (13) looks like a straightforward example of an assertion. There are no words or phrases that would mark it as an utterance used to perform some other kind of illocutionary act. It does not read "Imagine that two great yellow caravans..." So what we have here must be a case of indirect illocutionary action:

> In indirect speech acts the speaker communicates to the hearer more than he actually says by way of relying on their mutually shared background information, linguistic and non-linguistic, together with the general powers of rationality and inference on the part of the hearer.[191]

For example, I say to you, "Let's go to see *The Bicycle Thief* on the 28th," to which you reply, "That's my father's birthday." I understand your reply to be a refusal of my invitation. But how do I do this? Your utterance looks like an assertion. However, as a reply to an invitation, the mere assertion, "That's my father's birthday" is inappropriate. It violates the conversational maxim requiring relevance. I assume that you are participating in cooperative conversation with me and infer from this that what you intend with your utterance is in fact relevant as a response to my invitation. All it takes from here is a little logic on my part to infer that you are refusing my invitation. After all, shared factual information implies that you have obligations arising from your father's birthday that will interfere with your ability to come to the movies with me. Your assertion implies that you are unable to do what I invite. Thus you have effectively achieved your intention to get me to realize that you will not come to the movies by getting me to realize that you so intended. This might seem to be belabouring the obvious, but it is necessary to show it is that the apparent assertions of much fiction could actually be directives.[182]

This, then, is the theory. A work is fictional if it is used as a prop in a game of make-believe. A proposition, on the other hand, is fictional if there is a prescription to imagine it. And this is the case when the rules of the game, together with the work, generate a directive to imagine the proposition. What does this look like in an actual case? Take a book like *Tom Sawyer*. When read, this book is typically being used as a prop in a game of make-believe. The game has rules such as "Take this book to be a report of actual events" or "Read the sentences in this book as truthfully reporting events." This rule, along with the others operative in the game and the sentences in the book, generates a large set of propositions that are to be imagined. The reader is to make-believe that these propositions are all true. That is what it

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[181] J. Searle, "Indirect Speech Acts," pp. 31-2

[182] The invitation situation is not a perfect match for what must be going on with many fictional propositions. It is a case in which assertion succeeds and an additional, indirect speech act is also performed. But fictional cases are ones where often assertion fails while another indirect speech act is performed. This is admittedly a complication, but not a serious one. Irony shows how the complication can be handled. Searle treats irony as closely related to indirect speech acts, proceeding through the same kind of mechanism. For instance, I back your car into a wall and you mutter, "That's brilliant." I am not for a moment confused about the illocutionary force of your utterance. You do not assert that my action is brilliant, quite the opposite. This I know because the situation clearly makes the attribution of brilliance inappropriate. I know you don't believe that proposition. The simplest inference from this: You mean something like the opposite. You aren't asserting both "That's brilliant" and "That's really stupid", but only asserting the latter. So there is in principle no bar to saying one thing, meaning by it something else and *only* meaning something else by it. A example even closer in form to fiction is the following. An adult and child are eating together and the adult points at the child's plate saying, "Broccoli." Now, we all recognize this as the directive "Eat your broccoli." In the same way that this is achieved, (13) can be uttered to direct (14). When I recognize (13) as fictional, I recognize that I am to imagine this proposition; I may also recognize that (13) is not being asserted, but this is not necessary.

is for them to be fictional. The sentences in the book are the medium through which the directives to imagine are communicated. The work is a vehicle for these illocutionary acts.

As a fiction and the product of an author, a work is the vehicle for a series of directive speech acts. Those directives are requests to imagine or make-believe the content of the work. Normally, we recognize in a fictional work that the author is not asserting the content of the work but asking us to imagine it. Usually this happens even though the work contains no explicit directive utterances aimed at its audience. Fictional contexts have features that allow the recognition of fictional intent. Often cues are present in a work that indicate that assertion is not the (main) intended force of the utterances. "Once upon a time" is one such cue, having content that it would be absurd to assert may be another. But even in cases where there are no obvious internal cues to the fictional status of a work, aspects of the situation in which a work is encountered can get the point across. When I identify a proposition as fictional, I am claiming that its utterance is aimed at getting me to imagine it. Whether or not it is also being asserted is an independent question. The author may also assert some or all of the propositions that make up the content of a fiction. Some of them may even be true. Neither of these possibilities interferes with the fictional status of a work or the propositions it generates. The aim of a fiction, as a fiction, is to get the audience to imagine or make-believe. This is what makes it fiction. But this goal is consistent with also intending to get the audience to believe some propositions, even propositions that are part of the content of a work. As long as an author is directing the audience to imagine the propositions of a work, even if he is in addition genuinely asserting some of those propositions, the work is fiction.

Walton's objection to a speech act interpretation of fiction is threefold. Two objections concern the inclusivity of the account. Walton is worried that any linguistic treatment of fiction will leave out non-linguistic fiction. He also contends that there are authorless fictions that won't count as fictions for any theory of fiction based on speech acts. Thirdly, he thinks this approach gives the action of making fiction too fundamental a role in the nature of fiction.

However, Walton's dismissal of a speech act theory interpretation of fiction belies his own theory's continuity with the explanatory framework of speech act theory. It seems to me that he is too hasty in his rejection. I have already laid out how I think a speech act account of fiction can be drawn out of Walton's own theory. To make my case complete I must consider two kinds of fiction: non-linguistic works of fiction, and fictions without authors. My final task will be to address Walton's concern that speech act theory gives the production of fiction too central a role in the characterization of the nature of fiction.

My claim is that when a work is taken to be fictional it is taken to be the product of an author who is by means of the work performing illocutionary acts, possibly indirectly. In particular, the author is taken to be issuing a set of directives aimed at getting the audience to imagine various things. But while this might work admirably in the case of written fictions, it does not immediately seem to work for painting, sculptures, and so on. It does, however, work as well as Walton's own theory, as I will show.

Let's take up painting. Walton's theory says that for a painting to be fictional is for it to be used as a prop in a game of make-believe. In such a game there are principles of generation which prescribe, for instance, that certain patterns of paint on a surface represent certain things, and that what is represented is to be imagined. In effect, these rules assign propositional content to the painting and also prescribe that this content be imagined. As a result, there is some set of propositions associated with the painting. Together, the rules of the game of make-believe and the painting make these propositions fictional. Another way of putting this is that the painting is the vehicle for directing that these propositions be imagined. If this is right, then we have symmetry with linguistic fiction. The painting functions just as the words printed on the pages of a book. They are both the vehicles for directing that propositions be imagined.

Note that the core idea of Grice's theory - that the way in which meaning is communicated is by the audience recognizing the intention of the speaker to achieve the effect they aim at - is not restricted to linguistic communication. This notion applies just as well to any form of communication. Speech act theory does not preclude non-linguistic expression from being a vehicle for speech acts. Any media that can be used to express propositional content can be used to perform speech acts. When he objects that a speech act theory of fiction leaves out non-linguistic works, Walton ignores the broad scope of the theory.

Imagine a naturally occurring story. There is a large rock on which there are cracks in the shape of sentences that tell a story. Walton says of such a case that the

> ." ...realization that the inscription was not made or used by anyone need not prevent us from reading and enjoying the story in much the way we would if it had been... Some dimensions of our experiences of authored stories will be absent, but the differences are not ones that would justify denying that it functions and is understood as a full-fledged story.[183]

When being read as a story, the cracks in the rock are a work of fiction, but they are not products of an agent who is using them to perform an illocutionary act. This appears to be a problem for my proposal. I have claimed that what characterizes works of fiction is that they are vehicles for a particular kind of directive illocutionary action and here is a case that doesn't fit. There is no agent, so there cannot be any action.

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[183] K. Walton, *Mimesis as Make-Believe*, p. 87

But look at this case a little more closely. Walton wants to claim that you can take something to be the vehicle for a story even if you do not also take it that there is an agent using the object as a vehicle. But this cannot be the case. By his own theory, to read something as a story is to take it that there are prescriptions to imagine certain propositions. A bunch of scratches in a rock can *resemble* the words that are conventionally used to get you to imagine certain propositions. However, recognizing this is true of a bunch of scratches is not the same as reading them as a story. If you go beyond noting the resemblance to actually attributing fictionality to the propositions, you imply that there is a set of prescriptions to imagine the propositions that make up the story. In reading the scratches as a story you are taking them to be the vehicle for a request, a request to imagine certain propositions. And it is not the case that you can take something to be the vehicle for a request without also taking it that there is an agent intending that the thing is such a vehicle. A directive is an illocutionary act just like an assertion. If we cannot have one without an agent then how can we have the other without an agent? Walton's claim seems more plausible than it really is because of the way he phrases it, but the realization that an agent did not produce the cracks in the rock does, I think, prevent us from simply reading the story.[184]

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[184] A few commentators have expressed similar worries about Walton's contention that naturally occurring scratches in the rock face can be fiction. Currie says that "... the most Walton's argument could establish is that we may treat the shapes on the face of the rock *as if they were fiction*" ("Works of Fiction and Illocutionary Acts," p. 306) More generally, but along the same lines, Lamarque points out that even if not all props are made, they are all made into props. ("Essay Review of Kendall Walton's *Mimesis As Make-Believe*," p. 162)

I do not dispute that we can read scratches in rocks as telling stories. We can. I do not mean to imply that if we do such a thing we believe that there really is some agent who wrote the story. We don't. I do think that what is happening if we 'read the story' is that we are pretending that there is an author. It is important to emphasize that an author does not have to be an actual agent that produced the work. An author can be herself a fictional character. But even if only in this ghostly way, there is always an author associated with any work of fiction, an author who is present in the act of appreciating that work. In order to even read a work as having propositional content, if we take speech act theory seriously, we must be positing an agent with certain kinds of intentions, making use of the work to get us to recognize those intentions, and thereby certain effects on us as readers.

My job now is to apply this theory of fiction to mathematics, in particular to redevelop Kitcher's naturalistic account of mathematics. A speech act theory of fiction provides the tools for a constructive empiricist account of mathematics. A number of tasks were put aside in earlier chapters. From the first chapter, there is constructive empiricism's coherence problem: articulating the empirical content of theories without committing to the truth of what they say about unobservables. I argued this can be done by distinguishing talking about the world and talking about a theory. The make-believe theory of fiction gives us a way of doing this, especially now that we can see that utterances about fictions often appear assertive when in fact they are directive, and that the form of utterance and what is asserted with them are often quite different. Chapter two left the task of redeveloping Kitcher's naturalist philosophy of mathematics in a way that accounts of our talk about mathematical theories but is not committed to abstract mathematical objects. This task can also be accomplished with the help of my reformulated make-believe theory of fiction.

Chapter Four

Mathematics and Storytelling

Remember what we are looking for: an antirealist philosophy of mathematics in which disbelief in the entities mathematical purport to be about does not interfere with the legitimacy of the use of mathematics and all its distinctions. The resultant account of mathematics should incorporate the strands that I pointed to in chapter two—Carnap's distinction between internal and external questions, the formalist notion of mathematics as a game, Kitcher's mathematical empiricism—in a way that allows the full use of mathematical language and distinctions. The philosophy of mathematics must not deny the objectivity of mathematics, not restrict its use by science or logic; but equally it must not be realist about either abstract or fictional objects. Remember van Fraassen's claim that a constructive empiricist philosophy of mathematics must "be a fictionalist account, legitimizing the use of mathematics and all its intratheoretic distinctions in the course of that use, unaffected by disbelief in the entities mathematical purport to be about." And that within mathematics "the distinction between structure of different cardinalities and the nonisomorphism of real number continuum and natural number series are objective."[185]

With an appropriately acceptable theory of fiction to hand, I am now in the position to outline such a philosophy of mathematics. Together with elements of Kitcher's naturalism, the make-believe account of fiction can generate the constructive empiricist view of mathematics I have been looking for. By rejection of abstract objects it satisfies empiricist scruples. The make-believe theory of fiction shows how we can make sense of mathematics as fiction, as stories, and it connects theory and practice in mathematics with human experience of the phenomenal world. It also respects the constructive element in constructive empiricism, opening the way to see how mathematical theories can work as models of phenomena—human collectings and segregations and the mental and physical operations made possible by mathematical formalism as it develops—without requiring the full truth of the models. And by allowing the full use mathematical theories and all their distinctions, it maintains the objectivity of the distinctions and judgments made within mathematical theories.

Recall Kitcher's view. It maintains that mathematics starts with theorizing about the rudimentary physical and mental operations of agents in and on the world. From this theorizing, through a process of generalization and idealization, an agent emerges who is not limited by time and weakness, physical or mental. This agent does what we do, only more and better. The process also generates theories, mathematical ones like arithmetic. These theories are about the ideal agent. They are not true theories, since their subject matter is non-existent. But, because of their origins in the mental and physical operations of limited agents they are empirically adequate theories. In chapter two I discussed Kitcher's attempts to deal with the objection that his naturalistic account of mathematics merely substitutes one abstract object, the ideal agent, for another, mathematical objects. There I argued that his analogy with idealizing theories in science and use of the notion of vacuous truth does not adequately answer the objection, and his reformulation of the concept of truth for mathematics is an insufficient reply. I suggested that the fictionalist aspect of Kitcher's remarks could be developed to answer this objection and answer the semantic question. However, I put

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[185] B. van Fraassen, "Empiricism in Philosophy of Science," p. 283

off explaining exactly how this would work until after an exploration of the status of stories and fictions. It is now the time to take up this question. The make-believe theory of fiction considered and developed in the last chapter allows me to address these issues.

Storytelling is the central notion that can allow Kitcher's naturalism to avoid realism about (objectionable) mathematical objects. His overall strategy for avoiding belief in abstract objects is arguing that mathematical language, when it is properly understood, is not really about abstract mathematical objects. So, Kitcher changes the domain over which mathematical variables range—instead of abstract objects of some kind, mathematical statements quantify over the concrete operations that we perform in and on the world. His translation of first order arithmetic in the language of Mill arithmetic generates a set of axioms that quantify over operations. All the familiar results in elementary arithmetic can be proved with these axioms, and the first twelve axioms cause no trouble. Since these only require finite domains, they can be satisfied by actual existing physical operations. However, a problem arises when we consider all of the axioms. Axioms 13, 14, and 15 look like they require infinite domains:

13: $(\exists x)Ux$
14: $(x)(\exists y)Syx$
15: $(x)(y)(\exists z)Azxy$

These can not be satisfied except with infinite domains—their truth depends on them. So concrete operations cannot satisfy Mill Arithmetic; there is not an infinity of them. On the other hand, abstract operations could do the job. So abstract operations (or some other kind of abstract object of which there is an infinity) appear to be forced on Kitcher by Mill Arithmetic. Despite his attempt to avoid them, abstract objects turn out to be the real subject matter of mathematics. It looks like Kitcher's overall strategy for avoiding realism about mathematical objects is undermined by the need for infinities.

Kitcher has offered a few responses to this criticism, all involving the idea that mathematical theories are about an ideal agent rather than any actual agent in the world. I argued in chapter two that, with one exception, his responses are inadequate either making the wrong propositions come out true, or not providing a satisfactory account of truth. The exception, of course, is the idea that mathematical theories are stories. This idea needs development, but can be made adequate.

The suggestion is that mathematical theories are stories about an ideal agent. A theory like Kitcher's Mill Arithmetic is about an agent not limited as we are by mental and physical weakness:

> One way to articulate the content of the science is to conceive of mathematics as a collection of stories about the performances of an ideal subject.[186]

But this move alone does not automatically reinstate unproblematic truth for mathematical theories. If mathematical theories are stories about an ideal agent, then they can only straightforwardly be true if that ideal agent exists. Standard semantic requirements for truth don't just go away when we call a stretch of discourse a story. As was clear in the last chapter, we cannot expect to magically get truth from such a move unless we are willing to accept fictional objects. But fictional objects are at least as objectionable as, possibly more than, regular mathematical abstract objects. So the ideal agent should not be taken to actually exist if avoiding realism about abstract mathematical objects is the aim. The ideal agent might not be exactly a mathematical object, but she is, if she is anything, abstract and open to any of the objections that that status risks.

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[186] P. Kitcher, "Mathematical Naturalism," p. 313

Kitcher might be accused of holding an account of mathematics that is realist about the ideal agent, one that substitutes one kind of abstract object—an ideal agent—for another—numbers or sets or what have you. But this is clearly not the position he means to take, and it is certainly not one I would endorse in any case. Kitcher highlights this point in "Mathematical Naturalism," saying, "I have been taken to substitute one kind of abstract object (ideal agents) for another (sets). But, as I took some pains to emphasize, there are no more any ideal agents than there are such things as ideal gases."[107] So we cannot get automatic easy results from the idea that mathematical theories are stories about a real ideal agent. But we can get results if we make use of the make-believe theory of fiction.

Mathematics as Make-Believe

Kitcher's naturalism explains the applicability of mathematics. Reinterpreted with the make-believe theory of fiction, it gives an account of the status of mathematical propositions that does not require an ontology embracing abstract mathematical objects. This account allows for mathematical truth, knowledge, change and discovery. It has the further advantage of avoiding both psychologism and subjectivism—external reality determines the truth or falsity of mathematical propositions not just what we think. It also provides truth conditions for mathematical statements that really are truth conditions. This is accomplished by simply accepting standard semantics, if at the cost of judging many mathematical propositions to be false.

Several features of the make-believe theory of fiction prove helpful in achieving the desired results. The notion of 'fictional' it embodies, its ability to handle the bet-sensitivity of ordinary statements, its employment of speech act theory concepts—all of these aspects of the make-believe theory show how the idea of mathematics as stories, mathematics as make-believe, provides a theory without inflationary metaphysics. It allows us to embrace Kitcher's suggestion that mathematical theories are stories about an ideal agent without endorsing an ontology bloated with fictions. So I will take up Kitcher's suggestion that mathematical theories are stories, and use the make-believe theory of fiction to adapt it to the task of outlining a constructive empiricist philosophy of mathematics. This provides an antirealist philosophy of mathematics that has the features we need: it is fictionalist, without embracing fictional objects, but recognizes the objectivity of mathematical facts.

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[107] P. Kitcher, "Mathematical Naturalism," p. 324, n. 32

Mathematical theories are stories. As parts of a story, the propositions of any mathematical theory are fictional. Whole mathematical theories themselves, as stories, are fictional also. They are fictional works. The make-believe theory of fiction says that a work is fictional if it is used as a prop in a game of make-believe. It also says that a proposition is fictional if there is a prescription to imagine it. So mathematical theories are props in games of make-believe in which there are prescriptions to imagine the propositions that make up the theories.

When an utterance generates a fictional proposition, the utterance is not assertive but directive. Of course, an utterer can, in addition, be asserting some proposition(s) with an utterance, but it is the directive nature of utterances and their aim—getting the audience to imagine something—that constitutes the fictionality of propositions. A series of utterances together can produce a fictional work, for instance in the form of a book. Fictional books are typically used as props in games of make-believe. As fictional this is the use for which they are intended. The rules of a game of make-believe, together with the work, generate directives to imagine propositions. Mathematical works, then, are props in games of make-believe. And the propositions that make up these works are fictional. So games of make-believe associated with mathematical works prescribe imagining the propositions of those works.

The conjunction of the make-believe theory of fiction with the claim that mathematics is fiction entails that mathematical works are props in games of make believe. This may initially not look very plausible. Mathematicians and students of mathematics are not playing games! But consider what it really means in more detail. Take a fictional book like *Tom Sawyer*. According to the make-believe theory, when read as fiction, this book used as a prop in a game of make-believe. The game has rules such as "Take this book to be a report of actual events" or "Read the sentences in this book as truthfully reporting events." These rules, along with the others operative in the game and the sentences in the book, generate a large set of propositions that are to be imagined, fictional propositions. The reader is to imagine these propositions, to make-believe that they are all true. That is what it is for them to be fictional. The sentences in the book are the medium through which the directives to imagine are communicated. The work is a vehicle for these illocutionary acts, requests to imagine or make-believe the content of the work. If this is transposed to the case of a mathematics book, does it really become implausible?

Mathematical theories differ from standard fictions in a way that complicates matters. They are often not produced by a single author. Mathematical stories are in an ongoing state of composition and re-composition. They are not normally associated with a single author. And interest is focused on mathematical theories in a way doesn't include their origins in the same way that authors are included in the attention paid to standard fiction. This makes expressing some aspects of the account of mathematics as make-believe in speech act theoretic terms rather awkward. I can say that the utterances that produced Tom Sawyer were not assertive, but directive quite naturally. The author was not asserting the story, but directing his audience to imagine it. On the other hand, saying the same of mathematical theories is not so natural. There is often no one author whose utterances produced a story, but a series of people often involved in an extensive ongoing process of elaboration and revision. Nevertheless, mathematical theories are produced by authors, however much they might resemble a corporate entities spread out thinly both through time and space. So I will speak of the utterances of authors that produce mathematical theories.

Mathematical theories are stories so the utterances that produced them are primarily directive, not assertive. They can also be assertive, but whether they are or not is independent of their fictional status. It is dependent rather on different intentions of their utterer—not the intentions that make the utterances directive. For example, according to my theory we do not assert axioms 13, 14 and 15 of his Mill Arithmetic, but utter them as directives to imagine that they are true, to make-believe them. That this is the case depends on the intentions of the utterer satisfying the conditions that are laid down for directives, not assertions. These conditions do not include that the utterer believes (or has reason to believe) the proposition nor that the utterer intends for the audience to recognize that they believe the proposition. They require instead that the utterer wants the audience to comply with the directive and that they intend that the audience recognize this.

Axiom 13 is true though, so if Kitcher utters it perhaps he means to assert it as well as direct that it be imagined. That would just mean that more than one speech act was performed with the utterance, and there is nothing mysterious about this happening. Kitcher is not actually the author of Mill Arithmetic (as the name suggests he isn't), but we can still speak of him as standing in for the author. Kitcher at least *relates* Mill Arithmetic to us. In this act of storytelling, he is standing in for the author. An author engages in creative acts that generate a fictional work. These acts are utterances—however delayed their reception—and they are directive speech acts. But a storyteller, whether author or not, also performs directive speech acts in relating the story.

Make-believe and Hilbert's instrumentalism

Mathematical works are the vehicles for directive speech acts that aim at getting an audience to imagine certain propositions. These propositions are fictional in the works and in the games of make-believe authorized for them. But there are three categories of mathematical proposition: fictional and false, fictional but true and not fictional and false. You might think there is a fourth—not fictional but true. However, any true mathematical proposition should be part of the appropriate mathematical game of make-believe. And if it is, then there is a prescription to imagine it in the game and so it *is* fictional after all. This can be the case, note, even if no one has ever thought about the proposition or noticed that it is prescribed by the game. Once the rules of the game have been settled, so have all the propositions that are fictional in it. Just as establishing the rules of chess establishes the set of all possible games, including even those that have never been played and will never be played (if there are any such).

We are interested, in any case, in propositions of the two classes that are fictional. From the point of view of this division—finite true propositions on the one hand and false ones on the other—the make-believe theory begins to look a lot like one we have already briefly considered in chapter two: Hilbert's. Hilbert also divides mathematics up into two parts and insists that the infinite is not true but of only instrumental value. This much is shared by Hilbert and the make-believe theory: the acceptance of finitistic mathematics, and the rejection of infinite. That Hilbert's Program was (likely) undermined by Gödel's work makes this similarity dangerous. It is the very rejection of infinite mathematics as only instrumentally valuable that makes Hilbert's position vulnerable. It might do the same to the make-believe theory. But there are differences between Hilbert's view of mathematics and the make-believe theory. These differences can establish that even if we think that Gödel's second incompleteness result has shown that Hilbert's view of mathematics is untenable, this conclusion doesn't follow for the make-believe theory. To show this, I will first give a brief sketch of Hilbert's view of mathematics and his Program.

Hilbert aims "to establish once and for all the certitude of mathematical methods." Their certitude comes into question for Hilbert because while propositions of ordinary finitary number theory, like "1 + 1 = 2," can be known with certainty, mathematical statements that in some way involve infinities cannot. Hilbert calls the finite and infinite parts of mathematics 'real' and 'ideal' respectively. Real mathematical statements can be known certainly because they assert something about objects that we directly experience; they are about objects that are "immediately intuitable and understandable without recourse to anything else."[180] But ideal mathematical statements, since they involve objects that are not immediately intuitable and understandable, cannot be certainly known. Only by restricting mathematics to the finite could certainty therefore be maintained. But this restriction is not desirable, it wipes out too much of mathematics. In Hilbert's words, "No one shall drive us from the paradise which Cantor has created for us."[189] Hence "we must supplement the finitary statements with ideal statements."[190] And this supplementation necessitates a proof of the consistency of the resultant system, because "the extension of a domain by the addition of ideal elements is legitimate only if the extension does not cause contradictions to appear in the old, narrower domain."[191] Even if they are false, ideal statements can be used to establish real mathematical result, so long as they don't introduce falsehood into those results.

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[180] D. Hilbert, "On the Infinite," p. 196

In fact ideal statements are strictly meaningless for Hilbert since they are not about concrete objects intuitively present as immediate experience and capable of exhaustive survey. Ideal statements and methods are meaningless but have instrumental value. Their instrumental value comes from use as an inference ticket. But if ideal mathematics can take you from true real mathematical propositions to false real mathematical propositions then its instrumental value is lost. So ideal mathematics must be proved consistent. Any proof of this must use only finite methods, Hilbert thinks, because otherwise the proof itself would not be dependable.

So real mathematics deployed metamathematically is the heart of Hilbert's Program. It focuses on the formal properties of ideal mathematics and by treating it as a concrete object, using methods that employ only concepts that can be instantiated in perception, i.e. finite methods, it aims to prove its consistency. Approaching ideal mathematics as a concrete object whose formal properties are to be investigated is what makes Hilbert's view formalist. It is also what makes him vulnerable to Gödel's proof that for any formal theory T in which arithmetic can be formalized, if T is consistent then its consistency is not provable in T. Hilbert's Program demands a real mathematical proof of the consistency of ideal mathematics, but Gödel's second theorem shows there cannot be one.

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[189] D. Hilbert, "On the Infinite," p. 191
[190] D. Hilbert, "On the Infinite," p. 195
[191] D. Hilbert, "On the Infinite," p. 199

Gödel's second theorem must then also threaten to undermine the make-believe theory, given its similarity to Hilbert's Program. If there is not a finitistic proof of the consistency of infinite mathematics then mathematicians engaging in mathematical games of make-believe are on dangerous ground. They cannot be sure that what they are doing will not lead from true finite mathematical statements to false ones. Just as the Hilbertian cannot be sure that ideal mathematics is a reliable instrument for producing real mathematical propositions, so the make-believer cannot be sure that a game of make-believe can reliably produce true finite mathematical propositions from true finite mathematical propositions.

Despite the general similarity, however, the accounts do differ in several ways. Firstly there are many differences in the details of the accounts. I have been using pragmatic notions to evade the ontological commitments that mathematics seems to foist on us—claiming, for instance, that we do not assert that every number has a successor but merely direct that this be imagined while engaging in games of make-believe. It isn't clear that Hilbert would accept this way of going about things. For one, he may not even accept the semantic problem as it has been constructed. Detlefsen, for one, understands Hilbert to be opposed to conceiving the semantics for mathematics and metamathematics referentially.[132] If instead we conceive the semantics procedurally, as Detlefsen argues Hilbert does, the nature of the problem of infinite mathematics is transformed. The semantic problem in this case would simply go away, perhaps to reemerge in another form. So the main motivation for invoking the pragmatic dimension of language imagining and games of make-believe may not even have any force with Hilbert. There is also reason to expect a disagreement about the status of certain mathematical statements, very large finite numerical equations for example. I have focused on the limitations of the physical universe in claiming that mathematical statements involving infinities are not true, and these limitations imply that there are also false mathematical statements not involving infinities, namely those that require that the universe be bigger than it actually is. There is, in other words, a biggest number. And with this Hilbert would also disagree.

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[132] M. Detlefsen, *Hilbert's Program*, p. 42, n. 30

So where does this leave us? We have identified a few differences between the Hilbertian and make-believe accounts, but none of these seem relevant to the question of consistency. That problem remains. However, two arguments can be made that throw into question the seriousness of the Gödelian problem. One is general and comes from Michael Detlefsen, who has argued that Hilbert's search for a finitistic proof of consistency is not undermined by Gödel. The other begins with a difference between Hilbert's Program and the make-believe account, a disagreement over the need for certainty and the importance of a finite consistency proof.

Detlefsen's defense of Hilbert against the challenge Gödel's second theorem poses begins with the fact that more than one formula of a theory T may express the consistency of T. Gödel's formula, $\text{Con}_g(T)$, is merely one among many. It is generally agreed that every finitary truth can be expressed as a theorem of T. But from this it does not follow that every formula of T that expresses a theorem of the finitary metamathematics of T will be provable in T. It only follows that some such formula will be provable in T. And so, we can accept that Gödel's proof that $\text{Con}_g(T)$ is not provable in T, but hold out hope that some other formula expressing the consistency of T is provable. This hope is founded on the possibility that "the properties of $\text{Con}_g(T)$ which Gödel's proof calls upon to show the unprovability-in-T of $\text{Con}_g(T)$ may not all be included among those properties of $\text{Con}_g(T)$ which cause us to say that it expresses the consistency of T."[193] Detlefsen does not, however, rest on merely the hope. It might be possible to generalize Gödel's proof so that it shows that every formula which expresses the consistency of T is unprovable-in-T. Detlefsen argues that no current attempt to do so is adéquate and that there are reasons to believe that no such attempt can be successful.

The success of Detlefsen's argument would reestablish the possibility of a finitistic consistency proof. However, even if Detlefsen's defense of Hilbert's Program fails the make-believe theory is not seriously threatened by Gödel, because *it has no commitment to establishing the certitude of mathematical methods*. We can see why this is important by considering the expressed motivation for Hilbert's program. I started my brief outline of Hilbert's view of mathematics with the claim that he aims to establish the certainty of mathematical methods. Speaking in 1925, Hilbert says that "The present state of affairs where we run up against the paradoxes is intolerable... If mathematical thinking is defective, where are we to find truth and certitude?" but that there is "a completely satisfactory way of avoiding the paradoxes without betraying out science."[194] This way is, of course, the way of Hilbert's Program. "Operating with the infinite can be made certain only by the finitary."[195]

It would be nice—for everyone not just finitists—if there were a finite proof of the consistency of a formal system(s) containing elementary number theory. But no one has this certitude now. Not Hilbert, not the advocate of the make-believe account, not even the most liberal of mathematical realists. Realists and anti-realists are in the same boat with respect to consistency. Any proof of consistency must be made with methods more powerful than the system for which consistency is being proved. This means that any proof of consistency can be doubted—the methods it uses must also themselves be proved consistent. Obviously this threatens an infinite regress of consistency proofs. And undermines the possibility of certainty in a very general way.

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[193] M. Detlefsen, *Hilbert's Program*, p. 81

[194] D. Hilbert, "On the Infinite," p. 191

However, the make-believe philosophy is not Hilbert's philosophy. It starts from a more radical empiricism about mathematics than Hilbert. To the extent that there is mathematical knowledge, it is empirical and uncertain. The idealizing that produces recognizable mathematical theories does not somehow inject certainty into the proceedings. But this needn't worry us if we are not demanding certainty in the first place.

Mathematics as a Game

There is still in the make-believe account, however, a parallel to formalism that might cause protest. The make-believe theory says that mathematics is a game. It does not say that mathematics is merely the manipulation of meaningless symbols but it does say that it is a game. And this is a claim that might by itself be objected to.

One possible source of resistance to the idea that mathematics is fictional and mathematicians engaged in make-believe is the thought that games are somehow not serious. However, despite the fun we might have with them, exercises of the imagination needn't be trivial or without serious purpose. The analogy with children's games of make-believe should not be taken to imply that mathematics is in any way frivolous. Even children's games serve serious purposes—the development of needed skills, for example. Likewise, the fact that mathematics is associated with games, is said to *be* a game, does not mean that it is not a serious business.

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[195] D. Hilbert, "On the Infinite," p. 201

Another objection, voiced by Michael Resnik, is that "if mathematics were just a game or an art we could not explain its usefulness, because we do not *use* them in the way we use mathematics. They may teach, entertain or enlighten, but they do not supply premises for scientific and practical inferences."[196] But this seems too restrictive a view of games and art. And that this is the case is apparent from the thought behind Resnik's rejection: "...it strains the imagination to think that mathematics is an elaborate game, fable or art form that has just happened to prove useful."[197] Just happened to prove useful? Mathematical theories are stories, but they don't *just happen* to prove useful. They are intended to be empirically adequate, to apply to the world. That they do is no accident.

What sort of a game is mathematics? It involves working out the consequences of and relationships between various stories about an ideal agent who can perform operations without the constraints that we operate under. It is a game in which participants are encouraged to extend and elaborate on the story that has so far been told. Those people who participate most fully in the game of make-believe, mathematicians, extend the story by introducing new arguments and proofs, as well as problems and concepts. One rather interesting feature of this game that Kitcher points out is the way in which it generates its own content. Mathematical theorizing might begin with rudimentary physical and mental operations, but it does not end there. New language, new mathematical notation enables us "to perform new operations or to appreciate the possibility that beings released from certain physical limitations could perform such operations."[198] These operations themselves then become subject to systematization and idealization, which in turn may require the invention of yet more new concepts and notation.

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[196] M. Resnik, *Mathematics as a Science of Patterns*, p. 42

[197] M. Resnik, *Mathematics as a Science of Patterns*, p. 42

The idea, then, is this. Mathematical theories are stories. These stories are not (mainly) true, but fictional. When we learn them, discuss them, and even alter or extend them, we are not committed to their truth but only to their empirical adequacy. Up to the limits of the actual world, the limits of our actual abilities these theories are (meant to be or taken to be) true. Beyond that they are merely fictional: to be imagined. Like theories in the natural sciences as conceived by constructive empiricism, mathematical theories need only be empirically adequate.

Like any proposition, according to the make-believe theory, a mathematical one is fictional if there is a prescription to imagine it. So in a game of make-believe the participants are supposed to imagine the propositions of the theory they are engaging with. This raises a couple of questions. First, how does it get established which propositions are fictional for a particular story (theory)? An answer to this question would solve the generation problem for mathematical theories. Second, what is it to imagine a mathematical proposition? This question can be generalized to any fictional proposition, but seems particularly important in the case of mathematical propositions. Each question reveals interesting features of the make-believe account. The generation problem shows how the make-believe account can make sense of the debate between classical and intuitionistic mathematics. The question of imagination reveals (at least some of) what the account has to say about the activities of mathematicians.

Generation and Intuitionism

The problem of generation discussed in chapter three is not a serious one for mathematics. For regular fictions,

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[198] P. Kitcher, "Mathematical Naturalism" p. 314

stories like *Tom Sawyer*, there is a puzzle about how their content is generated. There appears not to be clear and precise general principles that govern the generation of the content of a story from the sentences that are inscribed in books. For mathematical theories this puzzle is also present, though it is perhaps a little less puzzling.

One of Walton's suggested generation principles—the Reality Principle—clearly holds for mathematical theories. The Reality Principle says that:

> If $p_1, \ldots, p_n$ are propositions whose fictionality a representation generates directly, another proposition, q, is fictional in it if, and only if, were it the case that $p_1, \ldots, p_n$, it would be the case that q.[199]

The principle is formulated this way to allow for inferences based on logic—if it is fictional in a story that Holmes always brings his umbrella when it is raining and that it was raining on the day he went to visit Gladstone, then we are allowed to infer that on the day he went to visit Gladstone, Homes brought his umbrella—as well as those based on physical laws—if it is fictional that Holmes lit his pipe with a match while visiting Gladstone, then we are allowed to infer that it is fictional that there was oxygen in the room during his visit. This principle can not hold universally, one reason being the existence of inconsistent fictions—they will cause an inferential avalanche if this principle is not limited. But the principle should hold for mathematical theories. There isn't concern about accommodating inconsistent mathematical theories, they are to be avoided. And in this context, logical inference does not produce unwanted extra material either; if it follows from the axioms, it is part of the theory. Indeed, we need more than just logical inference here, if mathematical induction is not a logical principle. But the need to include inferences involving physical laws seems less compelling here, though we can preserve it to maintain generality.

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[199] K. Walton, *Mimesis as Make-Believe*, p. 145

However, the main point to be made about generation for mathematical theories is that it is almost certainly closed under logical consequence. I say that it is *almost certain* that generation is closed under logical consequence for mathematics because of intuitionism. Intuitionists hold that mathematical theories are not closed under (classical) logical consequence, some theorems of classical logic are not theorems of intuitionistic logic. The Heyting calculus, for instance, is intended to be a restriction of classical logic. So the disagreement between intuitionist and classical mathematicians concerns what inferences are permissible in a mathematical context. The disagreement arises from the intuitionist view of mathematics as essentially a mental activity and belief that a claim that such-and-such a mathematical object exists is a claim that that object is constructible. So unless an object is constructible, no existence claim about it can be included in a mathematical theory. A mathematical object for which there is no construction, nor a proof that it is not constructible is then a counterexample to the law of excluded middle. If *p* asserts the existence of that object, then the intuitionist insists that neither *p* nor ~*p* is true. Also, constructible mathematical objects can easily be incomplete. They can have only those properties that are determined by their construction. Heyting gives the example of a constructible number that is neither not rational nor rational.[200] Notice here the connection with regular fiction.

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[200] Heyting gives the following example: "I write the decimal expansion of π and under it the decimal fraction $\rho = 0.333...$, which I break off as soon as a sequence of digits 0123456789 has appeared in π. If the 9 of the first sequence is 0123456789 in π is the kth digit after the decimal point, $\rho = 10^k - 1/3.10^k$. Now suppose that ρ could not be rational; then $\rho = 10^k - 1/3.10^k$ would be impossible and no sequence could appear in π; but then $\rho = 1/3$, which is also impossible. The assumption that ρ cannot be rational has led to a contradiction; yet we have no right to assert that ρ is rational, for this would mean that we could calculate integers p and q so that $\rho = p/q$; this evidently requires that we can either indicate a sequence 0123456789 in π or demonstrate that no such sequence can appear." Heyting, *Intuitionism - An Introduction*, p. 17

One of the peculiarities of fictional objects discussed in chapter three was just this sort of incompleteness. And recall, this can be accommodated by the make-believe theory quite easily. It isn't that the object is incomplete - since it doesn't exist, but the set of propositions that the game of make-believe prescribes to be imagined.

If we wish to accommodate the intuitionist, then, we can say that generation is closed under intuitionistic rather than classical consequence. I realize that an intuitionist would be reluctant to embrace this way of putting things—their realism about mathematical objects as limited as it is still does not deny the existence of the natural numbers. An intuitionist might reject the make-believe theory as mistaken about the nature of mathematical activity and maintain that his disagreement with classical mathematics is precisely over which mathematical objects really exist and what properties they have. But these claims are open to dispute.

After all what exactly counts as a construction? There is some upper limit on the natural number sequence that we can actually construct. Unlike finitely proceeding series, infinitely proceeding ones outrun the human capacity for apprehension of the particular. So the natural number series isn't really constructible for us. Elementary number theory then, which even intuitionists start with, already gets us into a theory that is strictly false. It must be an idealization of some kind. And what I am suggesting is that it is the kind of idealization that we have built into the make-believe theory. The disagreement between intuitionists and classical mathematicians is about the content of mathematical theories. Since mathematical theories are stories about an ideal agent, the disagreement it is over what the right story is to tell about the ideal agent. Classical and intuitionist mathematicians disagree about which propositions can be included in the story. They disagree about *who the ideal agent is*.

Imagining and Participation

Intuitionist and classical mathematicians differ over what the story is, over which propositions are to be imagined. But this difference is not a difference in what it is to imagine a proposition. Imagining a story is a familiar thing, but what is it to imagine a mathematical proposition? I do not have a general theory of imagination. This makes it difficult to spell out exactly what is happening when a mathematician participates in a game of make-believe that prescribes the imagining of certain mathematical propositions. Nonetheless, there are a few things that can be said on this topic.

'Imagination' suggests a visual model. This is, however, a mistake. Imagination is not purely, or even primarily, imagistic. When you imagine a camel, your imagining may come in the form of an image—a brownish, large four-legged, humped creature. But imagine, for a moment, the rich *ungulate* smell of a camel. Or the velvety texture of its muzzle. These, though not imagistic, are just as legitimatly instances of the imagination. They are imaginings of an object that is external to the imaginer. But while external things often are the objects of imagination, an imaginer herself often is as well. I may, for instance, imagine that a camel is chasing *me*. Imaginings about oneself can be from the inside or the outside. Imagining that a camel is chasing me could be from the perspective of a spectator observing the chase or from my own perspective in the chase.

When we imagine stories, imaginings of each of these kinds may play a role. As a narrative unfolds, its audience may merely be caught up in it as if they were an invisible spectator or the events that occur, but even this detached involvement entails the audience imagining certain things about themselves. There is more to the fictional content of a game of make-believe than just the content of the work of fiction that is a prop in it. Recognizing that *p* is fictional, and complying with this by imagining *p* seems to necessarily involve some kind of self-imagining. Christopher Peacocke proposes that imagining something always involves at least imagining, "from the inside, being in some conscious state."[201] Walton suggests that "all imagining involves a kind of self-imagining... the minimal self-imagining... of being aware of whatever else it is that one imagines."[202] When I imagine a camel, I am also at least imagining that I am aware of the camel. To that extent imagining a camel (or anything else) is imagining something about myself. And with a story as well, when I imagine a story I am also at least imagining that I am aware of what happens in it. But the imagining probably also involves a stronger reflexive engagement than this. If the story is successful, my imagining of it will involve imagining of myself that I am experiencing or perhaps doing various things—riding along on horseback in pursuit of the hounds or what have you. This involvement is part of the game of make-believe in which the story is a prop; it generates fictional truths about me that are part of the game even though they are not part of the work.[203]

The imagining involved in a mathematical game of make-believe, I suspect, is rather more like this self-imagining than it is like merely imagining external things. It is more like imagining running away from a camel than imagining a camel. In fact, I would take this even further. Mathematical theories are stories about an ideal agent. Playing mathematical games of make-believe, I suggest, often involves imagining that you are the main character in the story. In other words, you imagine that you are the ideal agent; you imagine you are doing the things that the theory says that the ideal agent does. This suggestion is partly echoed by something Kitcher says, though he does not put it in terms of make-believe or imagining. He describes the result of idealizing the operations we perform in the following way:

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[201] C. Peacocke, "Imagination, Experience, and Possibility: A Berkeleian view defended," p. 21

[202] K. Walton, *Mimesis as Make-Believe*, p. 29

[203] This might seem to cause a problem for the make-believe account specifying the content of a fictional work. If there are propositions about me that are fictional in a game of make-believe using a work of fiction as a prop, then doesn't that make those propositions part of the fictional work? No it doesn't. What is fictional in a work is the propositions that are fictional in *all* authorized games of make-believe in which it is a prop. The propositions that about me that are fictional in the games of

> [W]e obtain a perspicuous way of reflecting on our actual operations - that is, on our structuring of the world of experience - by making up a story, a *story in which we effectively treat ourselves as freed from certain well-known limitations.*[204]

So there is a sense in which we are ourselves the subjects of mathematical stories—ourselves as we would be if we were "freed from certain well-known limitations".

I have said that mathematical theories are stories about an ideal agent. Specifically they are stories about the operations that an ideal agent can perform in her world. Mill arithmetic is a relatively simple story about the physical and mental collectings, combinings and segregatings that the ideal agent performs. So, if we are playing a game of make-believe with Mill Arithmetic, we are supposed to imagine the activities of this ideal Millian Arithmetician. Put this way the imagining is described as if it is all from the outside. But the imagining that goes on is more involved, active, and self-referential than is implied by the locution 'imagining that every number has a successor'. The mathematician imagines of herself that she is performing the operations specified by the propositions.

With their imaginative activity, mathematicians obviously don't just pretend to be an ideal agent doing Mill Arithmetic. They extend mathematical story by introducing new arguments and proofs, as well as problems and concepts. This

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make-believe I play with a work will not be fictional in the games that others play, so they aren't part of the content of the work.

must be partly a matter of their attending to new aspects or in a new way to the operations that they are imagining they perform. But it is also a matter of seeing the possibilities for new operations made possible by current language and notation. Mathematical theorizing might begin with rudimentary physical and mental operations, but it does not end there. New language, new mathematical notation enables the performance of new operations.

Objections: Truth and Knowledge

There are, of course, more objections that can be made to the idea of mathematics as make-believe than I have dealt with thus far. In one way or another the ones I see all stem from discomfort with the claim that mathematical propositions are false. Some mathematical propositions just seem true, yet my account makes them come out false. Furthermore, we have mathematical knowledge, and this seems hard to reconcile with the falsity of mathematical theories, the non-existence of mathematical objects. Also associated with the falsity of mathematical propositions is the objection that the make-believe view cannot account for mathematical discoveries. That is, the account makes any developments in mathematics inventions rather than discoveries—a result that many judge unacceptable. Excessive freedom, or at least the appearance of excessive freedom, is the ground for another truth-related objection to mathematics as make-believe. If mathematical theories are just stories, untrue stories, then nothing seems to constrain mathematicians in their theorizing. They could just make up any story they liked, as novelists do. But we know that there is something about the mathematical theories that we do have that makes them right, and we also know that just any old story would not do. It looks like the make-

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[204] P. Kitcher, "Mathematical Progress," p. 530. My emphasis.

believe theory gets a whole bunch of things wrong about mathematics.

But the appearance of getting things wrong is just that: appearance. All of these concerns can be satisfactorily addressed by the make-believe account. In each case, the role that truth is thought to play is taken over by another attribute, fictionality. I consider these objections in turn.

"1 + 1 = 2" is just plain true. So begins one objection to mathematics as make-believe. And so, it goes on, are many other mathematical propositions. Mathematics as fiction and make-believe cannot account for these truths, at least not all of them, and this undermines its plausibility.

In response to this objection, it is important to first emphasize the independence of truth-value and fictionality. The make-believe theory of fiction explicitly tells us that the truth-value of propositions is independent of their status as fictional. Propositions of Mill Arithmetic conform to this as much as those of *In Cold Blood*. A proposition is fictional if there is a prescription to imagine it, and there can be a prescription to imagine a proposition whether it is true or false. Hence fictional propositions are not necessarily false. In addition to being fictional, they might actually also be true. A partial answer to the objection is clear at this point. The answer is that yes, some mathematical propositions are true: those that the concrete world or some subset(s) of it satisfy. Some mathematical propositions are true—the finite ones that are satisfied by the world or something in it. These we can successfully and justifiedly assert, and for them truth is a reason for wanting to say that they are true. Remember, the idea is that mathematics starts with the concrete physical and mental operations that we actually do perform in and on the world. One plus one does equal two. The world makes this so.

But still, one of the main reasons I have taken the position that mathematics is fiction is that agreeing it is simply true entails ontological commitments objectionable to empiricism. Although the make-believe theory allows for the possibility of entirely true fictional works, saying that mathematics is fiction and that mathematical objects are *merely* fictions entails that many apparently mathematical statements are false. The concrete world does not make all mathematical propositions true. At some point in the series of propositions ("1+1=2," "1+2=3,"… "1+n=m") a proposition is reached that is false because what it refers to does not exist. There are mathematical propositions like "1+n=m" which many would claim are just plain true, but with a sufficiently large n that the present view judges them false. And there are also mathematical propositions like "Every number has a successor" which the present account, for parallel reasons, does not allow to be true. This seems problematic.

There are, to be sure, many mathematical statements that are false, "1 + 1 = 4," for instance. Surely some difference between the falsity of this statement though, and the status of a statement like "Every number has a successor." Saying that they are both false risks blurring the difference between them. But the make-believe theory of fiction has the resources to account for this difference without saying that truth value is what distinguishes propositions like "Every number has a successor" from propositions like "1+1=4." Recall one of the puzzles of fiction that I drew attention to in chapter three: the bet-sensitivity of ordinary statements in fiction. One feature of a proposition like "Sherlock Holmes lived in London" is that, even if you think it is false because there is and never was a Sherlock Holmes, a bet against it will lose. This make sit difference from a proposition like "Sherlock Holmes lived in Toronto." A bet against this proposition would win. Both are false, on the make-believe theory, but differ with respect to bet worthiness. Bet-sensitivity will also provide an explanation of the difference between "Every number has a successor" and " 1 + 1 = 2."

Difference in truth-value would provide in some ways the simplest explanation we could have of the differing bet worthiness of propositions. In many cases it is truth-value that underlies this difference. But in the case of standard fiction, and, I would argue, also in the case of mathematics, relying on truth value to explain variation in bet-worthiness mires us in unacceptable ontology and problematic epistemology. But there are other explanations than truth-value. The make-believe account provides a good explanation of the bet-sensitivity of ordinary statements and this explanation is just as successful when transferred to the mathematical context. What distinguishes "Sherlock Holmes lived in London" from "Sherlock Holmes lived in Toronto" is the former is fictional in games of make-believe authorized for the Holmes stories while the latter is not. It isn't just plain true that Sherlock Holmes lived in London, but we might be tempted to think it is because of the rightness of saying it, of believing it rather than that he lived in Toronto. What distinguishes them is not, however, their truth values but their fictional status. Likewise, we can argue that what distinguishes "1 + 1 = 4" from "Every number has a successor" is that the latter is fictional in games of make-believe authorized for arithmetic while the former is not. When doing arithmetic, when involved in games of make-believe associated with arithmetic, it is appropriate to utter "Every number has a successor" while it isn't appropriate to utter "1 + 1 = 4." In arithmetic, all and only the propositions that follow from the axioms of arithmetic are to be imagined. "1 + 1 = 4" is not one of these. "Every number has a successor" is. Imagining one proposition is prescribed while imagining the other isn't.

Fictional works have authorized games of make-believe associated with them. These games generate fictional truths. Mathematical theories are fictional works, so there are authorized games associated with them. And so, as in 'standard' fiction, there are ordinary statements for mathematical theories: those statements that the authorized games prescribe to be imagined. This is what underlies the feeling that "Every number has a successor" is different from "1 + 1 = 4." Truth-value is not.

To *assert* that every number has a successor is a mistake. It is false that every number has a successor. However, it isn't a mistake, in the course of a game of make-believe authorized for arithmetic, to utter "Every number has a successor." The proposition "Every number has a successor" is fictional and, so, uttering "Every number has a successor" is perfectly appropriate. Conversely, it isn't appropriate in the course of such a game to utter "1 + 1 = 4". This proposition is not only false, it is not fictional, either. So the utterance is, in addition to being a mistake if assertive, inappropriate in the course of arithmetic game of make-believe. There is no prescription to imagine "1 + 1 = 4" in arithmetic. Uttering "Every number has a successor" while engaging in arithmetic could also be pedagogically appropriate. The utterance is a way of pointing out to a student or someone unfamiliar with formalized arithmetic what it is they are supposed to imagining. Situations such as these highlight another source of mistaken idea that mathematical propositions like "Every number has a successor" are just plain true. An utterance "Every number has a successor" may be performed to assert something. Recall Walton's claim about assertion and ordinary statements:

> In general, when a participant in a game of make-believe authorized by a given representation fictionally asserts something by uttering an ordinary statement and in doing so makes a genuine assertion, what she genuinely asserts is true if and only if it is fictional in the game that she speaks truly.[205]

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[205] K Walton, *Mimesis as Make-Believe*, p. 399

When engaging in games of make-believe, many utterances can be thought of as fictional assertions. In uttering "Every number has a successor" I do not actually assert that every number has a successor, but fictionally I assert this proposition. In the game of make-believe it is fictional that I assert that every number has a successor. But I may also be genuinely asserting something else, something that is true. Recall Walton's paraphrase of assertions made with ordinary statements while participating in a game of make-believe:

> To utter "Sherlock Holmes lived on Baker St.," is to assert "The Sherlock Holmes stories are such that one who engages in pretense of kind K in a game authorized for it makes it fictional of himself that he speaks truly" (where pretense of kind K is exemplified by the utterance of "Sherlock Holmes lived on Baker St.")[306]

Applied to the utterance in the course of playing a game of make believe authorized by arithmetic this paraphrase becomes:

> To utter "Every number has a successor," is to assert "Arithmetic is such that one who engages in pretense of kind K in a game authorized for it makes it fictional of himself that he speaks truly" (where pretense of kind K is exemplified by the utterance of "Every number has a successor")

And this is precisely true about arithmetic. The authorized games of make-believe associated with arithmetic make it fictional that every number has a successor. When someone engages in one of these games and utters "Every number has a successor" the rules of the game prescribe that it is to be imagined that they speak truly. In other words, the game prescribes that it is fictional that they speak truly, when they so utter.

The point of such an utterance being used to make the assertion, as I said above, can be pedagogical. It is sensible to make this assertion, and to make it in this way, when your goal is getting someone new involved in the game. Note that this is also a useful way of thinking about even expert mathematicians conversing about mathematics.

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[306] Walton gives paraphrases on p. 400-402 of *Mimesis as Make-Believe.*

Explaining a new result, or filling in a colleague on an area are in the relevant respects the same as teaching a neophyte. In all these situations the utterance of a sentence that can be used to directly assert a fictional mathematical proposition is a sensible straightforward way to assert that the proposition is fictional. Alternatively, it is also a good way to assert that in the game it is fictional that a person making such an utterance speaks truly. Why is this a good way of making these assertions? For one, it avoids awkward constructions. More importantly, it can get the point across without drawing too much attention to the pretense that being engaged in. Successful contributions to mathematics are hardly likely to come from engagement that is not pretty single-minded. Drawing attention to the make-believe nature of what is going on is liable to generate counterproductive disengagement. Using these utterance also encourages participation by modeling the behavior that constitutes participating in the game. For all these reasons it makes sense to make assertions in this way.

We can participate in games of make-believe, or we can merely talk about them. In other words, we can speak from an internal perspective or an external one. And this possiblity corresponds to Carnap's distinction between internal and external questions. As I said in chapter two, we should stay aware that there is a difference between asking from inside a theory, "Are there imaginary numbers?" and asking generally, critically, if there are any numbers. We can answer yes to the former question and no to the latter without inconsistency.

But what about mathematical knowledge? Knowledge requires truth; any propositions that are false cannot be known. So any view of mathematics that involves the claim that many mathematical propositions are false, especially uncontentious propositions like "Every number has a successor," is open to the objection that it cannot account for all of our mathematical knowledge. First notice that this objection is over expressed. The make-believe account I am arguing for does say that some mathematical propositions are straightforwardly true—those that make claims satisfied by concrete operations in the world. These true propositions can be known and constitute some of what we call mathematical knowledge, perhaps a great deal of it. But for many this set of propositions will not be sufficiently large. It just leaves out too much mathematical knowledge—knowledge of things like the fact that every number has a successor. Mathematics as make-believe makes a nonsense of such knowledge.

Our mathematical knowledge comes to us in at least two ways. Some knowledge about the concrete operations that we have comes from, as you might expect, regular everyday experience of the world. But the experience that gives us some other mathematical knowledge comes to us through experience of a different kind. Any mathematical knowledge that we about infinities cannot come to us through experience of those infinities, we cannot have such experience. It must come to us through the mediation of the theories that are constructed to systematize and idealize our actual operations. And this shows that the complaint against the make-believe theory is made too hastily. What looks like knowledge about numbers is really knowledge about theories or games of make-believe.

It is true that the account entails that "Every number has a successor" is false and hence we cannot know it. But it supplies a substitute: "'Every number has a successor' is fictional." This proposition is true. And we can know *this*. That is, we can know that arithmetic is such that "every number has a successor" is to be imagined, to be make-believed. In games of make-believe authorized for arithmetic, it is fictional that a person who utters "every number has a successor" speaks truly. It is our knowledge of this fact about games of make-believe, and our habit of asserting it by uttering "Every number has a successor," that give the impression that what we know is that every number has a successor. We don't actually know this at all.

One might argue, on the other hand, that calling mathematical theories stories imputes a problematic degree of freedom to mathematics. If mathematics is just something we make up, then we could in principle make up anything and call it mathematics. What is fictionally true in mathematical theories depends on the rules of the games of make-believe that are authorized for them. These are, however, merely games. We can change their rules or play different games, again producing different mathematical theories, different fictional truths. Are we not free to tell any stories we like? The idea of this much freedom does not sit easily with our intuitions about the nature of mathematics. Mathematics as we do it now is surely not *merely* one story among a whole bunch that we could have told. It is correct in a way that the freedom of making up stories does not appear to allow.

Mathematicians are not free to this degree, however, even if, as I have argued, what they are doing is making up stories. This is because mathematicians are not free to make up just any old stories. There are restrictions placed on the stories that mathematicians can tell. These come in part from the empirical origins of mathematics, our rudimentary operations. Mathematical theories aim to systematize these operations and this aim circumscribes the stories that can be told. The restrictions this places on mathematical theories also carries along as new theories are introduced and developed. Systematizing the operations that are made possible by the development of new notations imposes the same restrictions as systematizing rudimentary operations.

But a final truth-related objection to mathematics as make-believe is left to consider. Even if it is circumscribed by its own aims, mathematics is a game of make-believe. It is the product of human thought and ingenuity. Stories are invented not discovered. So mathematics must be invented, not discovered. But this conflicts with our inclination to say that mathematicians make mathematical discoveries, they don't make them up. Cohen discovered the independence of the Continuum Hypothesis, the Greeks discovered that there are an infinite number of primes, Andrew Wiles discovered a proof of Fermat's Last Theorem. In each case, these discoveries are of mathematical facts related to infinities in one way or another, as are most interesting mathematical discoveries. So they are not truths, and the make-believe account can account for as genuine discoveries about the world. If infinities are not out there, independent of us, it is hard to see how the account can say these were discoveries.

The main reply to this objection involves considering the nature of the games of make-believe associated with mathematics. These games are constituted by rules about which propositions are to be imagined or make-believed in what circumstances. Although they are in many respects much more complex, we can compare these games to the game of chess. Chess is constituted by a set of rules. These rules govern the initial placements and movements of chess pieces on a board, who gets to move when, what constitutes the end of a game, a win, a loss and a draw. Within these rules players are free to move as they desire. The game of chess is a human invention; we did not discover the game of chess. Nevertheless, there is room for discovery in the game of chess. Once the rules were decided on, the game of chess had been defined. But the mere invention of chess does not equal knowledge of all of the possibilities of chess. We might say that the possibilities are there to be discovered. Thus we can say that Evans discovered the Evans gambit, even though chess is an invention. Simply having created something does not mean knowing everything there is to know about it. We can make discoveries about our own inventions.

Discoveries of the sort I have in mind here are within a theory or game considered synchronically. For instance, arithmetic allows for the discovery of new theorems, facts and proofs. But room for discoveries of another nature is seen when mathematics is considered diachronically. Mathematics develops over time, not just through the discovery of new facts within the framework of a single theory. The theories and games of make-believe that make up the practice we call mathematics change and develop through time. And we need to be able to account for these kinds of changes, and have room to be able to at least sometimes call them discoveries. These can be discoveries of various kinds of things. These could be discoveries of new techniques to solve problems posed within or by already accepted mathematics, or they could be the discovery of new principles or concepts that help systematize theretofore unsystematized results.

Kitcher's account of mathematics is naturalizing and I mean to retain that naturalism. Naturalistic constructivism rejects a priori epistemologies for mathematics. This rejection is not undermined by my fictionalist extension; it is if anything underscored by it. Mathematical knowledge, like any, requires more than merely true belief. There must also be some kind of justification. Having mathematical knowledge is often a matter having evidence for a true mathematical proposition in the form of a proof. But proofs begin, ultimately, with axioms, and over the status of axioms Kitcher strongly disagrees with a priorists. Kitcher puts the case in the following way: We know mathematical axioms typically from books or blackboards or being told by a teacher whose authority we trust. These are the ways in which we learn mathematical axioms. However, a priorist philosophers of mathematics believe that these procedures cannot ultimately confer justification on mathematical beliefs. This method of belief acquisition does not generate knowledge—that is the exclusive domain of mathematical intuition, be it platonic or constructive, or stipulative definition. But this epistemology is rejected by naturalists in favor of a view that can make sense of mathematical knowledge as the product of an ongoing historical process. "Our present body of mathematical beliefs is justified in virtue of its relation to a prior body of beliefs; that prior body of beliefs is justified in virtue of a yet earlier corpus; and so it goes."[207] This chain of justification terminates in human perceptual experiences of operations in and on the world. New axioms and concepts are justified (or not) by their ability to advance mathematics by systematizing results already obtained or answering questions raised by currently accepted mathematical belief and practice.

This is a sketch of Kitcher's alternative to a priorist epistemology of mathematics. While it works for an account of mathematics that accepts the truth of mathematical axioms, things are a little more complicated for a view that takes many accepted axioms to be false.[208] If we believe that certain mathematical propositions are false, it is inconsistent to also hold that we have justified belief in them. Since I have argued that any mathematical axioms not confining themselves to claims satisfiable by concrete operations are false, I will have to adapt to this complication. Happily I can do this, with the help of some of van Fraassen's distinction between acceptance and belief. New axioms are justified in virtue of their ability to systematize and explain. According to Kitcher these are the reasons we have to believe new axioms. But the ability to explain and/or systematize is not reason to believe in truth, only a reason to accept. We accept new axioms and the systems that they embody, I suggest, not as true but as empirically adequate. Thus the proposal and development of mathematical theories is the same kind of process as the proposal and development of other scientific theories. Mathematics and science are one of a piece. In the place of Kitcher's formulation, we can say instead that our present body of mathematical propositions is justified in virtue of its relation to a prior body of propositions; that prior body is justified in virtue of a yet earlier corpus and so on.

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[207] P. Kitcher, "Mathematical Naturalism" p. 299

Acceptance and Paraphrase

This suggests a way of characterizing the constructive empiricist distinction between belief in and acceptance of theories: One facet of accepting a theory is the decision to engage in the games of make-believe associated with the theory. This decision implies neither belief nor disbelief in what a theory says, but implies, rather, a change in focus away from truth and towards the representational possibilities of a theory.

Accepting a mathematical theory means accepting all of its propositions, not as true but as empirically adequate. It involves the acceptance of false propositions like "every number has a successor" but not belief in their truth. Claiming this is not endorsing a new strategy for explaining intuitions we have about mathematical propositions or the utterances we make about mathematics or while engaging in it. What I suggest here is not an addition to paraphrase strategy borrowed from Walton, it is another aspect of it. The acceptance of a false proposition is what makes true the paraphrase that it can be used to assert. Accepting a proposition, true or false, means taking it to be fictional. When I accept the proposition "Every number has a successor," I join a game of make-believe partially constituted by the prescription to imagine that every number has a successor. That is part of what it is to accept arithmetic: a decision to engage in certain imaginings in accord with a particular game of make-believe.

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[208] I think that preserving this epistemology is one reason that Kitcher makes gestures towards a pragmatic theory of truth for mathematics.

In other words, accepting arithmetic means taking there to be a prescription to imagine its propositions. For a proposition like "Every number has a successor," this is just the same as agreeing that in arithmetic, a person who utters "Every number has a successor" makes it fictional of herself that she speaks truly. As a participant in the game of make-believe, the arithmetician makes certain things fictional of herself in the game by making certain utterances. And this exactly the true claim that the make-believe account says can be asserted with an utterance of "Every number has a successor":

> To utter "Every number has a successor," is to assert "Arithmetic is such that one who engages in pretense of kind K in a game authorized for it makes it fictional of himself that he speaks truly" (where pretense of kind K is exemplified by the utterance of "Every number has a successor").

We might say that accepting arithmetic is what makes the paraphrase true—it is because we engage in the games we do that the above paraphrase is true, and this is to say that it is because we accept the theories that are props in those games that it is true. Moreover, this link between acceptance and playing a game of make-believe is just what is needed to properly address constructive empiricism's coherence problem.

Coherence and Make-Believe

So we are now also in the position to return to the coherence problem raised for constructive empiricism at the end of chapter one: specifying the empirical content of theories in a way that does not require belief in the truth of a theory beyond what it says about the phenomena. Constructive empiricism says that theories are models, and empirically adequate theories are models that have substructures isomorphic to the observable world. So articulating the content of constructive empiricism—that acceptable scientific theories are merely empirically adequate—requires that we say what parts of the models of a theory correspond to the observable parts of the world, the phenomena. But when we say which of the substructures in a model are isomorphic to the world we will only be able to do this in theory laden language and that this language will have entailments about unobservables the constructive empiricist cannot embrace. So we will have sentences with references to unobservables entailed when we specify the empirical substructures of our models. And this seems to entail that merely by articulating what constructive empiricism says we should believe entails belief in claims that go beyond the limit of the phenomena that it establishes. Thus there is a challenge to the coherence of constuctive empiricism.

Van Fraassen characterizes a properly empiricist attitude towards science in the following way:

> To be an empiricist is to withhold belief in anything that goes beyond the actual, observable phenomena. To develop an empiricist account of science is to depict it as involving a search for truth only about the empirical world, about what is actual and observable.[209]

When we accept a scientific theory we are required to go no further in belief than the limits of what the theory says about what is observable, the limits of its empirical content. And fully acceptable theories need only be true to the limits of their empirical content - any truth beyond that is supererogatory. And this goes for mathematics as well.

According to constructive empiricism's semantic understanding of theories, theories are families of models. The linguistic expression of a theory is not directly interpreted in relation to the world. Interpretation proceeds in two steps:

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[209] B. van Fraassen, *The Scientific Image*, p. 202

> First, certain expressions are assigned values in the family of models and their logical relations derive from relations among those values. Next, reference or denotation is gained indirectly because certain parts of the model *may* correspond to elements in reality.[210]

Specifying the extension of predicates is a matter, then, of saying what they refer to in the models. Both observational and non-observational predicates and terms will have as their extensions elements and sets of elements from the family of models. A theory is empirically adequate if all the phenomena fit into one of its models. Consider the claim "There are 10^{23} molecules of H_2O in the glass." This claim, in conjunction with a large body of theory, is either observationally adequate or not. If I say that it is, what I mean is that is everything that I can observe about the glass and its content fits into (at least) one of the models corresponding to the claim and its attendant theory. There is a model, in other words, in which all of the elements of the theory exist and have the right relations to one another and this model has a substructure that is isomorphic to the glass, to what it contains, and to all possible observations of the glass and its contents.

The coherence objection appears to boil down to this argument. Internalism says that we cannot talk about what is observable and what is not observable except in vocabulary that perforce includes references to unobservable entities. Even if we can figure out what is observable and what is unobservable, according to our theories, inside a game of make-believe, there comes the point where we have to say what things in the world the theory is about—in other words what we mean when we say a particular theory is empirically adequate. This cannot be done except in theoretical language, and theoretical language is bound to have entailments that contravene the constructive empiricist restrictions on belief.

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[210] B. van Fraassen,, *Laws and Symmetry*, p. 213-14

In order to spell out what constructive empiricism says we should believe, we engage in talk that entails belief in unobservables.

When we accept a theory we let our language be guided by its models, by a certain picture of what the world is like. But in doing this we are playing a game of make-believe—pretending that the world is the way the theory says. When we talk about the models we are not necessarily talking about things that we believe to exist. This talk is how we establish what the empirical content of a theory is, from within a game of make-believe. We can do this without actually making assertions. This is what we are doing when we talk about the models, assigning elements of the model to expressions in the language and working out their logical relationships. But we can do all this without any ontological consequence because we only making-believe that we are talking about something existent when talking about the model.

Conclusion

The notion of a game of make-believe makes outlining a constructive empiricist philosophy of mathematics possible. As I have described it this philosophy of mathematics does not deny the objectivity of mathematics, not restrict its use by science or logic; and it is not realist about either abstract or fictional objects. It is "a fictionalist account, legitimizing the use of mathematics and all its intratheoretic distinctions in the course of that use, unaffected by disbelief in the entities mathematical purport to be about." [211] While engaging in a mathematical game of make-believe, a mathematician has available to her the full use of mathematical language and its distinctions. No abstract objects are necessary to support the objectivity of our mathematical knowledge, and disbelief in the entities mathematical purport to be about does not interfere with the legitimacy of the use of mathematics and all its distinctions. Further, the naturalism adopted from Kitcher explains what the true portions of mathematics are about and why mathematics is useful. It is useful precisely because it is an idealization of actual concrete phenomena: operations. All of this occurs within the framework that constructive empiricism itself provides—the epistemological limitations it mandates. Mathematics as make-believe also illustrates the importance of the pragmatic dimension of our theories, our explanations, and of our relation to the language we use, an importance central to constructive empiricism itself.

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[211] B. van Fraassen, "Empiricism in Philosophy of Science," p. 283.

As I said in my introduction, the conclusion that mathematics is make-believe may strike some as preposterous. My project may lead them to a negative conclusion: the make-believe theory of mathematics provides one more good reason to reject constructive empiricism. But I think my response to that bears repeating here. That the account links the human representational activities of science and art and mathematics seems to me an advantage. That it allows us to recognize more dimensions to our relationship with the language we use to make our way through the world than the two of belief and disbelief strikes me as a greater one. As does the recognition of the of the fundamental role of imagination and make-believe in mathematics and science.

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